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ART. I.—*Lectures on Physical Diagnosis.* By J. R. BUCK, M.D., Lecturer on the Theory and Practice of Medicine in the Louisville Summer School of Medicine.

LECTURE SECOND.

Before proceeding to a description of the physical signs of disease as derived from the conformation of the chest, or inspection, auscultation, and percussion, it will be proper to refer briefly to certain rules to be observed in the application of these different means of diagnosis.

1st. Inspection:—To inspect the chest to the best advantage, the patient should be in the erect position, arms hanging loosely by the side, and the chest entirely bare. It is customary with many physicians to make all examinations of this cavity through a thin garment. This, in the female, is to prevent exposure, and in the male to prevent the physician's face, in auscultating, from coming in contact with the filthy

bodies of his patients. I have seen practitioners use for this purpose a silk handkerchief. To the use of any garment in inspection of the chest there is an insurmountable objection; it is not only necessary to examine its general conformation, but also the condition of each particular region: a garment however thin spread over the chest would only come in contact with the most projecting points, while the limited contractions or depressions, would entirely escape observation. From the condition of the intercostal spaces, we are sometimes enabled to distinguish between diseases of the thoracic cavity: if for example, with a certain group of physical signs, the intercostal spaces on one side, are bulging, beyond the level of the ribs, the disease is seated in the pleural cavity, either hydro-thorax or emphysema; but if with the same group of signs, these spaces be entirely natural, corresponding on the two sides, then the disease is confined to the substance of the lungs. The whole chest then, being entirely exposed, the physician can take in at a glance both sides, comparing the projections and depressions upon one side with the corresponding points upon the opposite side. The patient should be directed to take a deep inspiration, and then to breathe rapidly that the mobility of the chest may be tested also. In certain diseases patients breathe almost exclusively with the diaphragm and abdominal muscles, while in certain other diseases the respiratory movement is performed alone by the ribs. Any bending of the body either to the one side or the other, would interfere with the examination.

Percussion.—If the patient is unable to sit erect, care should be taken to have every thing moved from around the body, that would tend to obscure the sound; the bed-clothes must be removed; the body does not sink down in a matress as it does in a feather bed—the former is therefore preferable.

Percussion is divided into mediate and immediate; by mediate percussion is meant, striking the body through the medium of some instrument, which instrument is called a pleximeter; by immediate, striking directly upon the body

with the fingers; the latter mode is never practised at the present day. The physician should so place himself that all his movements will be perfectly free, then adjusting the pleximeter accurately to the surface so that every part of it will be in contact with the body, it should be struck two or three times in rapid succession; by suffering the fingers to remain in contact with the instrument, the sound is obscured; the stroke should be given perpendicularly, and with the ends of the fingers. Percussion should be made alternately on the two sides, from the apex to the base of the chest, that you may have the advantage of comparison. The force to be used will depend upon the degree of muscular and adipose development over the organs to be examined; posteriorly it must be much more forcible than elsewhere, in consequence of the dense muscles and bone through which the sound must pass; a slight stroke would only elicit the dull sound of the muscles and bones, while a harder stroke would elicit a clear resonant sound from the organs within the cavity; in front, percussion must be a little more forcible in the precordial region where the lungs overlap the heart than elsewhere. It need never be sufficiently forcible to give pain.

Auscultation.—In auscultating the anterior portion of the chest, the body must be erect with the shoulders and arms thrown back, so that the muscles may be rendered tense and therefore better conductors of sound; in percussion, the soft parts may be pushed aside by the pleximeter, so that nothing but the integuments intervene between the instrument and the bony walls of the chest. In examining posteriorly and laterally, the position recommended in percussion is most favorable. The position of the patient is not more important than that of the physician; if he has to stoop with his head lower than his body, or bend over the patient in bed, the rush of blood to the head, or the irregular muscular contractions will produce certain sounds that may be confounded with those formed in the chest.

Auscultation like percussion is divided into mediate and immediate; mediate is the application of the ear through some

medium to the body. Immediate is its direct application. The medium more generally employed is a small trumpet-shaped tube made of some light wood, called a stethescope. This mode is greatly preferable to immediate auscultation; 1st. That in applying the ear directly, the whole side of the face is brought in contact with the patient's body, and the hair rubbing against the body causes a friction sound not unlike those occasionally formed in the lungs. 2d. In the examination of negroes or of filthy whites, the direct application of the face would be very unpleasant. 3d. In the examination of females it is not practicable thus to apply the ear, for even if not restrained by feelings of delicacy the mammary glands would prevent it; by using the stethescope, however, these can be pushed aside, so as to allow the mouth of the instrument to be brought in contact with the walls of the chest proper; and lastly, *cæteris paribus*, the sound is rendered more intense, a larger number of vibrations are collected in the expanded extremity of the instrument and conveyed in a condensed form through the narrow extremity into the ear.

The instrument held between the thumb and middle finger so that they will at the same time be in contact with the chest, must be accurately adjusted. No opening must be allowed to exist between the mouth of the stethescope and the body of the patient; the sounds would not only escape, but extraneous sounds would be admitted. Then, having the ear-piece accurately adjusted to the ear, the auscultator must concentrate his whole attention upon the sounds.

Having now described the characters of the healthy sounds of respiration and the manner of examining the chest by inspection, auscultation, and percussion, I come next to speak of those sounds which are produced by disease.

In the study of the morbid respiratory sounds, those cases are preferable which present these sounds in the widest deviation from their normal characters; thus in the advanced stage of phthisis, in extensive and acute pneumonia, in pleurisy, with large deposition of lymph, and in acute and severe

bronchitis, you have a group of symptoms so intense, that they can be distinctly heard by one who would fail entirely to appreciate the slighter alterations from the healthy state; still in describing these alterations I shall commence with the slightest.

The healthy respiratory sounds may either be altered in degree or changed in character by disease. In pneumonia a part of the lung being impervious to the air, the remaining portion compensates for it by taking on increased action; hence the sounds are louder in the healthy part, than they were previous to the attack of pneumonia; the same is true in regard to tubercular disease of the lungs, pleuritic effusions, in a word, to all diseases that impair the functions of any part of the lungs. If the disease be very extensive, this increased intensity may amount to the puerile respiration, a variety, as you know, healthy in the infant but diseased in the adult. The respiratory sounds are also diminished by disease; but this never occurs without an alteration in the relative loudness of the blowing and vesicular sounds. I am well aware that this opinion is at variance with the writers upon this subject, who teach that both sounds are lessened, equally, in emphysema of the lungs, and in pleuritic effusions. A moment's reference, however, to the anatomical structure of the lungs, will show the erroneousness of this opinion. The external or vesicular portion of the lung in which the vesicular murmur has its seat, is much more compressible than the internal or bronchial portion, where the blowing sound is formed; hence in emphysema, hydro-thorax, &c., the second sound is often obliterated entirely, while the first is heard distinctly, which certainly indicates that there is from the commencement of this effusion to the obliteration of the vesicles, a disproportion between the alterations in the two sounds. In emphysema, the disease is confined almost exclusively to the vesicles of the lungs, these are distended and relaxed; any alteration, therefore, in the respiratory sounds must be confined to that element which is formed in this diseased portion. While in emphysema then, the vesicular murmur is

not heard at all, the blowing sound is but little if at all changed. In that variety of emphysema common to old persons, the blowing sound is also very materially enfeebled, but this is doubtless owing to that general decay which is taking place in all the organs of the body, and the consequent diminished energy of their functions. Another class of alterations consist essentially in a want of proportion between the blowing and vesicular sounds; in this class are embraced rude respiration, bronchial respiration, tubal respiration, and cavernous respiration: another variety has been described, called the amphoric; this is but a variety of the cavernous, however, and may come very properly under that head.

Rude Respiration.—This consists in a feebleness of the vesicular murmur, with a slightly increased loudness of the blowing sound—both in inspiration and expiration—it occurs in consequence of the partial obliteration of the vesicular structure of the lung; in phthisis, for example, the tubercular deposit is at first very slight, causing a partial compression of the minute tubes and vesicles. The sound is therefore less in this compressed portion than natural, while the adjacent bronchial portion acts a little more energetically to compensate for the diminution in the compressed part; the bronchial or blowing sound is also increased from the tubercular deposit being a better conductor of sound than the healthy pulmonary structure.

Bronchial Respiration.—When the deposit increases so as to obliterate the vesicular portion of the lung entirely, the blowing sound is still farther increased, while the vesicular murmur is absent of course. The respiration being confined entirely to the bronchial tubes, is called bronchial respiration; any disease that will obstruct the passage of air into the vesicular portion of the lung will cause this sound.

Tubal Respiration.—This is a sound almost peculiar to pneumonia; it is heard when a large portion of the lung, including some of the larger bronchial tubes, is in a state of solidification. The only sound heard is that made by the passage of air through these tubes, the walls of which are

solid and unyielding, it resembles that made by the blowing into a flute with all the holes closed, or into any other solid tube.

Cavernous Respiration.—As the name implies, this sound is formed in a cavity or cavern in the lungs, it is therefore almost peculiar to phthisis, although it may sometimes occur in pneumonia; here also the second sound is absent, while the first is very much changed in character. It resembles the sound made by blowing into a narrow necked vessel, the neck represented by the bronchial tube and the body by the cavern; the character of the sound is little changed with the size of the cavern, which has caused the addition of another sound before alluded to. To the production of the cavernous respiration it is essential that there be free communication with the bronchial tube, and that the cavity be empty. It is generally heard at the summit of the lungs—this being the point where tubercular deposit first occurs. This sound bears some resemblance to that last described; they can generally be distinguished by their locality. The tubal respiration always occurs over the region of the larger bronchial tubes, while, as before stated, the summit is the usual seat of the cavernous. Again, the tubal is heard over a much larger surface; the cavernous being in all cases limited to the part directly over the cavity.

Rhonchi.—The sounds which we have just described involve necessarily some change in the substance of the lung, while the rhonchi, with but one exception, depend upon disease in the mucous membrane lining the air passages. These sounds are much more easily distinguished than those which depend alone upon alteration in the natural respiratory sound. The rhonchi, rales or rattles, for these terms I shall use indifferently, are divided into the dry rhonchi, and the humid or wet rhonchi. The dry rhonchi are formed by the passage of air through the bronchial tubes in a state of inflammation, or when the mucous membrane is thickened, or coated by thick tenacious mucus, which is not dislodged during respiration;

under this head there are two varieties, the sonorous rhonchus and the si blant rhonchus.

The sonorous rhonchus is produced by the friction of the air against the mucous membrane in a state of congestion and dryness; there are two conditions necessary to the production of this sound—diminution in the caliber of the tubes, and roughening of their surfaces. These conditions al-always exist in the first stage of acute bronchitis, in chronic bronchitis, when the lining membrane is thickened by deposits of lymph beneath it, and in some rare cases when the expectoration is so viscid as to adhere firmly to the sides of the tubes. This sound has been compared to the cooing of a dove, hence it is sometimes called the cooing sound; it resembles also very much the clear ringing noise made in snoring. It is formed in the larger bronchial tubes, and is heard both in inspiration and expiration.

Sibilant Rhonchus.—This only differs from the other in degree, it is a sharper, shriller sound, resembling a low whistle, and is formed in the smaller bronchial tubes, in the first stage of inflammation. The next rhonchi are formed by the passage of air through the bronchial tubes containing some fluid. They consist in the cavernous rhonchus, the mucous sub-crepitant, and crepitant.

The *Cavernous Rhonchus*, like the cavernous respiration, is formed in a cavity; but to produce the first, the cavern must be empty and to produce the last it must contain a fluid. This sound is sometimes called gu-gling from its peculiar character; the word will give some idea of the sound. In almost all cases it occurs in cavities from tubercular excavations; hence its usual seat is in the summit of the lungs. It is caused by the passage of air through a thick homogeneous mass, in a circumscribed cavern.

Mucous Rhonchus.—This rhonchus is characterized by an irregular bubbling sound; when we blow through any hollow tube into a liquid, bubbles are produced, and the breaking of these bubbles is attended with a slight noise. The character of this noise will vary with the consistence of the liquid. In

bronchitis when the inflammation is recent and not intense, a thin watery mucus is secreted; as it advances the fluid becomes whitish, opaque, and a little more tenacious; and still later it is much more tenacious and of a yellow color; the air passing through these different fluids, causes a succession of irregular bubbles extending over a very considerable surface, called the mucous rhonchus.

The *Sub-Crepitant* bears the same relation to this sound that the sibilant rhonchus does to the sonorous. It is formed in the smaller bronchial tubes and only differs from the mucous in degree. The bubbles are rather sharper; it requires a very practiced ear to distinguish between them.

Crepitant Rhonchus.—This sound is peculiar to pneumonia; it is formed by the passage of air through the minute tubes and vesicles of the lungs containing viscid mucus; it has been compared to the sound made by rubbing a lock of hair between the fingers close to the ear, or to the crackling of salt when thrown upon hot coals. It is generally mentioned as peculiar to the first stage; it may however be heard in the second or third stages, in the second or hepatizdd stage, this sound ceases where it was originally heard, but the presence of the diseased mass excites inflammation in the surrounding tissue, and in this the crepitation may be heard. The metallic tinkling or new-leather or creaking sound, does not come properly under the heads mentioned. The metallic tinkling is formed in a large cavity containing both air and liquid, and having also a communication with the external atmosphere. It may be produced in three ways; 1st, by a drop falling from the top of the cavity into the liquid beneath; 2d, by decomposition and disengagement of gas in the portion of the cavity beneath the liquid; and lastly, the air passing through a bronchial tube into this cavity below the level of the liquid.

Voice.—If the ear be applied to the chest of a healthy person while speaking, a confused roaring sound is heard; this is equal at all points, save the summit of the right lung, where

it is naturally slightly louder than elsewhere, owing to the anatomical arrangement of the bronchi.

If the lungs become solidified at any point, the voice is much more distinctly heard there, as it is then a much better conductor of sound than in a healthy state when almost the entire bulk is made up of air. When tubercles are first deposited, a slightly increased loudness or resonance of the voice is heard at the point of deposite. This increase is not very great, however, as the solid matter is small, and the air still enters the minute bronchial tubes around which it exists.

If the solid matter exists in sufficient quantity to obliterate entirely the vesicular portion of the lungs, as in the more advanced stages of phthisis, or in pneumonia, &c., then this alteration in the voice is very great, and is called bronchophony.

If a cavity exists in the lungs lined by a false membrane, and empty, not only is the intensity of the voice very great, but the articulation is also quite distinct; the sound seems in this case to come directly from the chest into the ear, as though one spoke into the mouth of the stethescope, while applied to the ear. It is called pectoriloquy. While the presence of this sound always indicates the existence of a cavity, its absence by no means proves that one does not exist, for if it contains mucus or pus, or broken down tubercular matter, pectoriloquy is not heard.

A circumscribed dilatation of the larger bronchial tubes may emit a sound very similar to this, but they may generally be distinguised by the locality. Pectoriloquy being heard almost uniformly at the summit of the lungs, where tuberculous excavations usually occur, while the sound emitted by a dilated bronchus would only be heard in the region of these tubes.

Amphoric resonance has been applied to an indistinct pectoriloquy extending over a larger surface, and formed by the excavation of an entire lobe of the lungs.

I shall notice but one other alteration of the voice—egophony. This is a tremulous vibratory sound, formed by the passage of the voice through a thin stratum of liquid, and is therefore heard in the first stage of pleuritic effusion.

ART. II.—*On the Use of Mercury in the Treatment of Southern Fevers.* By LAFAYETTE CARR, M. D., of Aberdeen, Mississippi.*

There is perhaps no subject connected with practical medicine upon which the sentiments of physicians are undergoing a more rational change than the exhibition of mercury in our Southern fevers. If the fact were not so I should probably incur the charge of presumption in expressing some of the opinions contained in this essay. I do not mean, however, to exclaim against the judicious employment of the remedy, for none can be more fully impressed with a sense of its utility when used with proper discretion, than I am. But my design is simply to point out the circumstances which, as I conceive, render its use improper, and likewise to show in what manner it has been abused. It cannot be denied that to meet certain indications in the treatment of fever, mercury is an admirable remedy. This is a fact which the experience of every day in practice confirms; while it is equally plain that its exhibition under other circumstances is preposterous and calculated to do mischief. In those cases of fever that seem to depend on torpor of the secretory organs alone, nothing can be more admirable than the action of calomel or blue mass in exciting them to a healthy exercise

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* The author of the following Essay has been engaged for several years in the practice of physic in Mississippi, and his experience in the use of mercury in the fevers of that region was embodied in an inaugural dissertation submitted to the Managers and Faculty of the Medical Institute of Louisville for the degree of doctor of medicine.—EDS.

of their functions, and whenever we find suppressed secretion unconnected with excessive excitement, we may confidently expect beneficial effects from its operation. But such is not very often the case in our climate, for here we scarcely see a case in which suppressed secretion is unconnected with excessive excitement either local or general, and either the one or the other of these circumstances, I apprehend, may constitute an objection to its employment. For it has appeared to me that much of the inordinate action of the medicine is produced by the inflammatory nature of our fevers; consequently the propriety of using mercury in their treatment previous to depletion, is at least questionable. It is customary to prescribe mercury as well in excessive excitement of the secretory organs as in torpor of those organs, but this practice is of exceedingly doubtful propriety. Indeed, it seems to me an absurdity to employ the same agent to meet such opposite indications in practice. True, by its secondary operation as an evacuant, it will sometimes answer our expectations, but these are cases in which the excess of local action is not very great, or is unaccompanied by that disposition to inflammatory action in the system at large, which usually marks our fevers. They are therefore to be regarded as an exception to the general rule, and in most of these cases perhaps a sufficient impression could be made on the secretions and our purpose fully answered by the exhibition of some of the milder cathartics. For we should not limit our views of the operation of mercury to its action upon the secretions alone, but bear in mind that it is a stimulant which affects every fibre of the system; and one thing has impressed me with the force of truth, that when mercury is given in the commencement of our fevers without previous depletion as a preliminary step, it will generally do harm. It is too common to prescribe mercury to restore the secretions without a due consideration of the circumstances attending their suppression; and whenever this course is pursued in the commencement of our fevers, it will most commonly be attended with disagreeable results. So far from restoring the secretions when

given in cases marked by excessive excitement, it commonly deranges them still more materially by increasing the general excitement, and often by the development of excessive local irritation in the organs upon which it specifically acts. These diseases appear to throw the system into that state of morbid irritation that is peculiarly favorable to the development of inflammatory action, which action, mercury by its universally stimulating effects rarely fails to produce, and being generally accompanied with a preponderance of morbid action in one particular part, the whole irritating effects of the remedy are diverted to that part, and thus the disposition of the disease to locate itself is confirmed. We often see fevers wherein the liver appears to be already in a state of great irritation with a superabundant secretion of bile, and in such fevers nothing is more common than to prescribe calomel forthwith, and without a more attentive examination of the case. At our next visit, perhaps, we shall find the system thrown into the most violent commotion; the skin and conjunctiva become icterode, copious discharges of vitiated bile take place by stool, and yet the fever and sufferings of the patient are greatly increased. If the use of the medicine is still persisted in with a view of exciting ptyalism, high inflammatory action is developed in the salivary glands, tongue, mouth, &c., which not unfrequently ends in mortification and death. In such cases venesection ought certainly to precede the use of mercury, but the propriety of its exhibition may be questioned even after this precaution, for if we bleed in the first place to remove irritation, either local or general, why should we the next moment prescribe an agent that will induce it again?

Nor is its administration in our congestive fevers exempt from the same necessary precautions. Here excitement is so intimately and mysteriously connected with debility that the operation of every remedy is perhaps attended with uncertainty. Much of the mischief which results from the employment of this agent is no doubt owing to culpable negligence in permitting it to remain in the system until it has been ab-

sorbed. Now congestive fever as it occurs with us is a disease in which purgatives must be used with extreme caution, and if the practitioner has unwarily given mercury to a considerable extent with a view of restoring the secretions by its action, he not only fails to accomplish this object but often finds himself in an embarrassing dilemma, to clear the system of the medicine before absorption has taken place, and at the same time observe a proper degree of caution in his measures for the safety of his patient. Sometimes it is true when the stomach and bowels are not particularly implicated, the remedy will restore the secretions and relieve him from embarrassment, but such instances are of unfrequent occurrence, and where those organs are chiefly concerned calomel, by its local irritation, will only increase the disease.

But it is in fevers characterized by high mucous irritation throughout their course, that the injudicious employment of mercury is most to be regretted, because here its effects are masked by the insidious type of the disease. The injurious effects of mercury upon the stomach and bowels when given in considerable quantities even where no previous disposition to disease existed in these organs is a well known fact. From this we may infer that the practice of giving it in alterative doses throughout the course of a disease marked by a subacute inflammation of these parts, is preposterous. That it is so, a careful observation of cases treated in this way will convince us. The first operation of the medicine seems to be beneficial, and this is the more unfortunate as the physician deceived by the partial cleansing of the tongue and abatement of the fever, thinks that a continuation of the medicine alone is necessary to effect his purpose. But soon the tongue becomes still more dry and red, the fever rises a grade higher, and the patient grows more irritable, and here the physician is apt to commit another mistake by presuming all this irritation to be caused by the inveteracy of the complaint, and discovering neither soreness of the mouth nor fœtor of the breath, he continues to exhibit mercury until the appearance of an eruption on the skin and mucous membrane discloses

his error, or the patient sinks exhausted by the insidious irritation of an agent that was thought to afford him the greatest chance for recovery. In such cases it seems to create a morbid irritation in the system, at the same time that it irritates and perverts the tone of the stomach and bowels.

Formerly it was thought that to secure the safety of the patient it was only necessary to induce salivation, and accordingly calomel was given in the most extraordinary quantities and frequently with the most deplorable results. This liberal and profuse use of a violent mineral poison having received the sanction of physicians became the common practice not only of physicians themselves, but unfortunately also of others entirely ignorant of its therapeutic agency, and was followed, as a matter of course, generally by serious and sometimes by fatal consequences. Hence the popular prejudice against it at this time, and hence that predilection for quackery and empiricism which seems to be characteristic of the age. We now understand that the substitution of one disease will not always remove, but will sometimes aggravate an existing one, and thus we may account for the failure of mercury to cure in many instances that seem to indicate its use.

The abuse of a remedy can form no valid objection to its judicious use, but it is evident that there has been and still continues to be an abuse of this remedy which the interests of science as well as of humanity require should be corrected.

Louisville, Ky., March, 1845.

ART. III.—*A Case of Diseased Kidney, with the Post-mortem appearances*, abridged from the case-book of the Hospital of the Kentucky Penitentiary. By W. C. SNEED, M.D., of Frankfort, Ky.

Glass, ætat. 26, of small stature, pale complexion, light hair, engaged by the keepers as a wagoner, was taken very ill on the night of the first of October, 1844. He complain-

ed of violent pain in the right side, and had considerable irritability of the stomach. The day previous to his attack he had eaten very freely of apples and drank a large quantity of sweet cider. He attributed his attack to this cause, and the steward of the hospital treated him accordingly. He had an emetic administered and afterwards a cathartic which brought off, each way, large quantities of undigested apples and cider; some relief was obtained, but it was only temporary as the pain again returned with increased violence, and persisted with more obstinacy. Venesection was resorted to, and about 24 ounces of blood taken from the arm. This, with the use of antispasmodics and opiates, gave him a truce which lasted until the evening of the next day. On the return of the pain we were called to him, and after making thorough examination we had no hesitation in declaring the case one of colic, brought on by the too free indulgence in apples and cider. There was evident tympanitis in the right side which we supposed to be owing to collection of wind in the cæcum and ascending colon. Percussion gave the sound of tympanitis, which with the other symptoms and facts in the case led us to believe, without any hesitation, that our conjectures were well founded. We resorted to the use of opiates and tartar emetic, and in a few hours succeeded in subduing the pain. His pulse during the remission of pain would be full and hard, but on the recurrence of pain, it would become extremely small and rapid. When the pulse was full the skin would be hot, and *vice versa*. The irritability of the stomach ceased in a day or two, and did not recur during the remainder of his illness. The violent symptoms gradually disappeared, and in the course of a week he seemed to be in a fair way to recover. The side, however, continued to enlarge very gradually, and by the end of the second week the abdominal parietes had become elevated above a level with the ribs. The enlargement gradually extended towards the left side, and by the end of the third week had crossed the median line about one and a half inches. Percussion now gave a much flatter sound than it had heretofore. His bowels

could be moved without difficulty, and his appetite had become very good. His stomach was perfectly quiet and performed its functions without difficulty.

It occurred to us about this time that our diagnosis was probably incorrect, and that the enlargement depended upon some other cause. We made some enquiries of the patient in regard to his previous history, and learned from him that when a boy he had received a kick from a horse on the right side, and that there had been considerable enlargement of the side ever since. This statement induced us to believe that the enlargement might depend upon disease of the kidney but we could not tell how it could be so rapid in its formation, and increase to such an immense size.

The enlargement continued to increase, and the patient to emaciate, until the thirty-fourth day after his attack, when he expired.

Sectio-cadaveris—24 hours after death. The body extremely emaciated; the muscles soft and flabby; a general petechial eruption over the surface. The chest presented nothing very remarkable. The heart was atrophied, pale and soft.

Abdomen.—On opening the cavity we were surprised at finding the viscera all crowded over into the left side. The liver, forced from its normal position, occupied the epigastric and left hypochondric region; it was somewhat atrophied but otherwise healthy.

Stomach about half its natural size, but healthy. The cæcum and ascending colon were on the left side of the vertebral column, and attached to the immense sack which proved to be the right kidney degenerated and filled with pus. This enormous sack was attached at its upper portion to the liver and diaphragm; on the left, to the mesentery, large and small intestines; posteriorly it was fastened down by adhesion to the spinal column; and in the same manner to the right side, and to the abdominal parietes as far over as the median line. At its lower end it was attached to the brim of the

pelvis, and a portion of it occupied about one-third of that cavity. No vestige of urethra could be found. The left kidney hypertrophied and healthy.

The sack contained a few ounces over two gallons of pure bland pus. The pelvic portion of the kidney seemed to have undergone the greatest change. It was immensely enlarged, and must have contained nearly the whole amount of the pus. On the inner portion of the sack there were several bony deposits, some of them very firm and as large as a thumb nail. Seven orifices large enough to admit a finger passed from the body to the pelvic portion of the sack. The holes answered to the places of the papillæ and were not connected to each other. The parietes of the sack were not thicker than the human skin, and were filled with veins and arteries. A preparation was made of the specimen, and is now in the possession of Professor Cobb, of the Medical Institute of Louisville.

Frankfort, April 1845.

ART. IV.—*A Case of Empyema successfully treated by an Operation.* By W. C. SNEED, M.D., of Frankfort, Ky.

Empyema is defined by writers to be a collection of fluid in the pleural cavity, whether it be water, pus or blood. Pus being the most common, and that which is the most fatal in effects, is the one to which the term is most usually applied. This effusion may take place into the pleural cavity from various sources, and the lungs from their yielding structure readily give way to it. Vomica of the lungs, abscess of the mediastinum, or phlegmonous abscesses of the lungs and liver, may gradually, by perforating the tissues which separate them from the pleural cavity, discharge their contents into it, which they seem peculiarly disposed to do, from the tendency to a vacuum produced in the thorax during the act of inspiration. A subacute inflammation of the pleura partaking of

a latent character, in which the sympathies are so obtuse as to give no evidence of its existence, or an acute inflammation which in its early or later stages ceases to excite pain and to disturb the action of the organs involved, instead of disappearing entirely, may degenerate into the chronic form, and thus give rise to a morbid secretion and deposit within the pleural cavity. The pleura partakes of the character of a mucous membrane, and when thus affected discharges, *gradatim*, a purulent fluid analogous in its character to that discharged in leucorrhœa, gonorrhœa, &c. In both the latter instances the secretion may be exhausting to the system, but is never directly fatal, in consequence of the secretion being carried off through the natural passages, and hence all the disadvantageous consequences are comprised in its formation. In the former they there only commence. The retention of the fluid is the danger to be dreaded, and constitutes the essence of the disease termed empyema.

In common pleurisy there is always a greater or less amount of serum secreted in the course of the disease, and deposited in the pleural cavity, which is clearly distinguishable by any one accustomed to the use of the stethescope, by the characteristic sound of egophony to which it gives rise. This secretion is usually removed by absorption during the recovery of the patient, leaving the thorax as free as before the attack. But in other cases, and where the disease is but illy cured, or the patient is too soon re-exposed to cold, a purulent secretion takes the place of the serous, and may accumulate in a large quantity in one of the cavities of the chest.

When collections of this character occur in one of the pleural cavities, the lung is compressed and retracts towards the spine, gradually yielding to the effused fluid that space which it formerly occupied. The vessels of the lung can no longer admit the fluids which filled them in a natural state. They become obliterated, adhere together, and the surface of the lung unites to the pleura costalis. The lung continues to diminish in size, and its internal adhesions multiply in pro-

portion to the increase of the effused fluid. Finally, the substance of the lung disappears entirely; the bronchial pedicles become contracted, and the air cells totally obliterated. The lung grows incapable of being dilated, and if the effused fluid is evacuated the space which it occupied remains vacant. Nature being unable to fill up this vacuum by the expansion of the lung adopts other means to overcome the existing difficulty. When the disease has advanced to this stage we may expect but little from an operation. If, however, it is performed at an earlier period, and before the foregoing condition has supervened, it may be attended with success. Nature in some instances becomes her own surgeon, and forms an opening through the pleura pulmonalis into the bronchi of the lung, and the matter is discharged by expectoration. Relief obtained in this way may only be temporary, for the air in respiration finding its way into the cavity of the chest becomes a source of irritation, the oxygen acting upon the pus, and the patient in a short time sinking from exhaustion.

Another process of nature, often attended with success, is a disposition to point externally, and form an opening through the parietes of the chest. This is nearly as effectual as a surgical operation, with the exception that there is more danger of admitting the air, which taking the place of the fluid becomes a source of irritation in the manner before explained.

Sir A. Cooper says that he has never known a case to recover after the operation, and Dupuytren asserts that in more than fifty cases in which he had operated, not more than two had recovered. Later writers than the above have, however, given more favorable reports, and many cases are recorded by English surgeons in which the operation has been attended with success. (See *Cyclopædia Prac. Med., Art.* Empyema.)

In reporting the following case we introduce many details which must be tedious and uninteresting to those who have had an opportunity of witnessing similar cases, and have made

themselves familiar with the treatment of this form of disease. But since no case of the kind has ever been reported in any Western journal, that we have any knowledge of, and as it was one of extreme interest to us, we feel disposed to give our notes of it just as we wrote them during its progress, believing that in this way we shall render it most instructive.

William Milam, aged 13 years, was attacked with pleuropneumonia on the 8th day of March, 1844. On Sunday, the 11th, we saw him and found that his attack was one of extreme violence, and required the prompt use of antiphlogistic remedies. He had been bled by his father, and had taken a dose of calomel on the day previous to our visit. We prescribed calomel in small doses combined with ipecacuan, and solution of tart. ant. in quantities to nauseate and keep down the febrile action. We saw him every day until the 18th, when the violence of the symptoms seemed to be subdued, and he gave every evidence of a speedy convalescence.

During the progress of his case we placed a large blister on the chest over the diseased lung, and kept it open by slightly irritating dressings, until we supposed his lung had sufficiently recovered to admit of its drying up. This seemed to have a powerful effect in checking the inflammation, and we hoped by keeping it open that it would, by its continued irritating effects, promote the speedy resolution of the lung and cause absorption of whatever effusion might have taken place in the pleural cavity. His case was aggravated and protracted by the existence of a large number of intestinal worms, which caused delirium, subsultus, &c., until they were expelled. By the proper administration of vermifuges, upwards of sixty ascaris lumbricoides were expelled, affording much relief and entire composure to the patient.

The case having progressed, and as we supposed, terminated, so favorably, we were much surprised on being summoned to visit him again on the first of April following. We found him laboring under the following symptoms, viz.: slight difficulty of breathing, with a hacking cough; pain in the left

side; no expectoration, and considerable acceleration of pulse—about 120 in a minute.

The blistered surface it was found on examination had not healed up. Through and around the blistered surface there were a great number of small boils, very tender and exceedingly annoying to the patient. Ordered a starch poultice to the blistered surface, calomel and ipecacuan to subdue the excitement, to be followed by sulphate of morphine to lessen pain.

April 2. The difficulty of breathing still continues; pain in his left side when he coughs; respiration hurried; pulse 130 per minute, quick and small; lies on his left side altogether; no appetite; has 84 small boils on the left half of his body and limbs, some of them are the size of a small acorn, but the greater number are very small and painful, some have matured and burst, but the remainder are red and hard. They are more numerous in and around the blistered surface than elsewhere. We discovered no enlargement of the side, and did not suspect effusion of any description within the pleural cavity. Same prescription ordered.

April 5. Found him free from pain in the side, and much easier than when last visited, in consequence of the entire disappearance of the boils. They have left no other evidence of their existence except a small, hard, bluish looking spot. His side is evidently swollen, but the swelling seems to be confined to the skin and subcutaneous cellular tissue. There is considerable œdema under the blistered surface; the skin there red, and rather warmer than in other portions of the body; his general symptoms much improved; appetite good; bowels moved without medicine; skin moist; pulse not so frequent. Continue the anodyne.

7th. Since our last visit no material change has taken place in his general symptoms; the œdema has increased and now occupies a large portion of the left side; the skin is very red, and there are several hard spots about the centre of the swelling; the intercostal spaces are pointed at several places; side is evidently enlarged, and all the symptoms go clearly to

prove the existence of a fluid in the pleural cavity; there is complete dullness over the whole of the left side; neither vesicular nor bronchial respiration can be heard from any portion of the lung. On measurement the side is found to be three inches larger than the opposite.

All the evidences of empyema being present, we had no hesitation in deciding upon the course proper to be pursued; but as we were not prepared to perform the operation, and the patient being six miles from town, we were obliged to postpone it until next day.

8th. Found the patient free from pain, cheerful, and in every way in a condition for the operation. The left side has increased in size since yesterday; breathing rapid but not difficult; the impulse of the heart is in the right side opposite its proper location; pulse 140; skin soft and pleasant; tongue clean; lies on his left side altogether; when turned upon his right side, becomes greatly distressed, and has to be turned back immediately.

In performing the operation we adoped the plan pursued by Dupuytren and Baron Larrey, which consists in making a valvular opening through the skin, by which means the air is prevented from entering the cavity of the chest after the fluid is evacuated.

We had the patient placed upon a trunnel-bed, that being the most convenient arrangement we could make, and punctured the side between the sixth and seventh ribs as near the angle, as the margin of the latissimus dorsi would permit. The skin being drawn upwards as tight as it could be, an incision about three-fourths of an inch long was made through it and the upper margin of the rib felt for. The lancet, which I used in preference to a trocar, was then gently passed into the cavity of the chest, and upon its withdrawal pus followed in great abundance. An assistant kept the orifice open by pressing the two edges apart, and the pus was suffered to flow out gradually as the expansion of the lung took place. The patient became exceedingly alarmed and we were compelled to close the orifice before we had taken more than

half a pint of pus. The skin was then drawn down and the orifice completely closed with an adhesive strip without the introduction of a particle of air.

11th. The patient being composed and every thing favorable, we introduced a silver female catheter and drew off half a gallon of pure bland pus. This gave marked relief and he expressed himself greatly relieved from the difficulty of breathing and weight in his left side. His cough which had been more or less troublesome, ceased almost entirely, and he could turn on his right side without the sense of suffocation which he had before experienced so violently.

The heart could be felt under the sternum, and respiration could be distinctly heard in the upper portion of the lung. Not more than a few bubbles of air entered the cavity on the withdrawal of the catheter. In all the lower portion of the lung there was no sign of respiration, except a peculiar sound produced by the expansion and contraction of the organ, somewhat resembling the sound made by shaking a cask partly filled with water. This sound we attributed to the agitation of a portion of the pus left after the catheter was withdrawn.

The orifice was again closed by adhesive strips, and a bandage was applied round the chest and drawn as tight as he could well bear it. He was put to bed and ordered tincture opii sufficient to relieve pain and produce sleep. Strong hopes were entertained that the bandage would contribute much towards bringing the ribs down to their proper position, and that the lung when relieved of its pressure would expand and speedily reoccupy its former space. This expectation was to some extent realized; on our next visit we found that the ribs had descended to a considerable extent, and that the lung had expanded at its upper portion so as to be distinctly heard.

Pus, it may be proper to remark, when thus collected in the chest, unlike the serous effusions, is never or scarcely ever removed by absorption. The successive increase of the

fluid reacts upon the membrane, which forms it, and thus keeps up a perpetual irritation and a perpetual flow. If not withdrawn from the body, it compresses the lungs against the mediastinum, and destroys the patient by suffocation, or wears him out by the irritation. In this case suffocation would have resulted in a few days, and the patient must have perished but for the timely removal of the fluid.

16th. Found patient cheerful and much improved since our last visit. His side has contracted and is but little larger than the opposite. His appetite is good and his food digests well; his bowels have been evacuated daily without the use of medicine; His skin is moist, but he has no cold stage or fever during the day. Respiration can be heard at the upper portion of the lung anteriorly, but not posteriorly. The incision has closed by adhesion and requires some force to pass the catheter into the chest.

Introduced the instrument and drew off three half pints of pure bland pus, without the introduction of any air. Ordered bandage to be continued and to be tightened as fast as the contraction of the chest took place. Continue tr. opii *pro re nata.*

29th. Visited patient every 2d and 3d day since last report and drew off about three half pints of pus each visit. His side continued to contract slowly, but the contraction has taken place posteriorly more than it has anteriorly. This we suppose has been in some measure owing to his resting on that portion of the ribs more than on the anterior. His appetite continues good, and his digestion undisturbed. He is greatly emaciated and when the pus is suffered to collect in larger quantities, he has chills and hectic fever. His heart is still in the right side and beats so forcibly as to be felt between the ribs. The right side has increased in fulness, and there is increased resonance in the right lung. This increased resonance has resulted from the extra amount of service required to be performed by this organ. The pus is becoming somewhat fœtid, and is filled with flakes of white matter which clog the instrument and render the extraction of the

pus much more tedious and difficult. The ribs from the contraction of the side are in contact with each other, and the passage of the catheter between them is very troublesome.

In consequence of the sinking down of the ribs, the orifice has lost its valvular character and now passes directly into the cavity of the chest. We determined consequently to let it grow up and make another, after the plan pursued in the first instance by which the introduction of air, now unavoidable, would be prevented, and this was accordingly done and the difficulty obviated.

May 2d. Drew off nearly two quarts of pus, more fœtid but thinner than heretofore. General condition of the patient nearly the same as at our last visit.

6th. The matter has continued to form and is rather on the increase. It has become exceedingly fœtid and much thinner than it was at first. His father has introduced the instrument and drawn off every other day about three pints of fœtid pus. The rapid and continued secretion of pus has caused emaciation to progress very rapidly, and the patient is not able to walk across the house. His appetite continues good, and his food digests without difficuly. The fulness in front continues, and the instrument can be passed about three inches in the direction of the apex of the heart; pulse 140; skin hot and dry.

We have been giving him wine, tinct. mur. ferri, quinine, &c., but perceiving no good to result from their use they have been discontinued. He has been taking a generous diet, which being well digested has supported the immense waste of the system and kept him alive.

9th. The pus has been accumulating since our last visit, and has distended the side and pressed against the diaphragm, until the abdomen is considerably enlarged; introduced the instrument and drew off two quarts of thin pus; after evacuating the matter he expressed himself relieved from the pressure on the stomach, of which he had been complaining for several hours. His breathing was much improved, and his feelings in every way greatly alleviated.

We discovered several days since a collection of air under the pectoralis major of the left side, which has continued to increase until it is now occupying a space as large as the palm of my hand. When pressed upon it disappears, but returns immediately on removing the pressure. When the pus is withdrawn from the thorax the emphysema disappears, and does not return until the cavity is again filled with pus. There is a communication from the thorax to the under portion of the pectoral muscles, which has been produced by an ulceration through the intercostal muscles.

We have used a great variety of medicines with the view of keeping up the tone of the system, and to arrest, if possible, the immense secretion of pus. So far as we have been able to judge, nothing has been of material service to him except the tinct. opii, which has relieved pain and produced sleep.

14th. His father has introduced the catheter night and morning and drawn off each time on an average about half a pint of pus. He has been allowed animal food freely. His strength has increased under the use of it, and in every way he seems much improved. The character of the pus continues about the same.

The instrument has to be passed about two and a half inches upwards and inwards before the pus will flow out. There is evidently a false membrane formed within the pleura costalis. The emphysema still exists when the pus is suffered to accumulate, but disappears when it is drawn off.

We continued to visit our patient once or twice a week during the months of May and June, and pursued about the same course of treatment as that above detailed. We discontinued the use of all kinds of medicine and relied entirely upon wine and a generous diet to support the system. The matter was drawn off daily, and often twice a day, by his father, averaging from one to three half pints per diem. The bandage was kept on and gradually tightened as the side contracted. Under the use of the wine and a generous diet he gained strength notwithstanding the copious secretion of

pus, and by the first of July he could walk across the house without help.

July 1st. He is much improved in every way. The pus is not so fœtid nor secreted in such abundance; it is more transparent, and has now more the appearance of serum than of pus. The emphysema has nearly disappeared, and the side has contracted until it is about two inches smaller than the opposite one.

The absence of all inflammatory symptoms and the change in the character of the secretion induced us to believe, that much might be effected by the use of astringent injections into the pleural cavity, and we determined, notwithstanding the doubts expressed by the highest authors upon the subject, to try their efficacy in the case of our patient. Before resorting to this remedy we learned that a medical gentleman in an adjoining county had been the subject of the same affection in his youth, and we addressed him a letter inquiring as to the treatment pursued in his case. In reply to our inquiries he addressed us a letter, which is subjoined, giving a brief history of his case, and recommending very strongly the use of astringent injections.

We commenced injecting about half a pint of a weak decoction of oak bark into the cavity once a day, after having first drawn off the pus. The result was truly gratifying. From this time he improved rapidly; the secretion lost its fœtor, and became almost serous in a few days. It continued to be secreted, however, until about the middle of August, when the amount became so small and serous that we discontinued the injections, and let the orifice close. The emphysema gradually subsided after we commenced the use of the injections, and entirely disappeared before the orifice closed. His left side is at this time two and a half inches smaller than the right. The spine is curved to the right about three-fourths of an inch. His left shoulder instead of sinking, as I anticipated it would, is higher than the right: this may be attributed to the fact of his lying on his left side altogether during his illness. The heart is now under the sternum and can

be felt about as distinctly in the left as in the right side. Respiration can be distinctly heard through a greater portion of the left lung, and the metallic tinkling which has existed so long has ceased entirely.

The gratifying results of the treatment of this case of empyema—results so seldom met with when paracentesis is performed, appear to be due, in part, to the youth of the subject, and to the great recuperative powers of his system. But much we think is due also to the valvular opening which was made, and which kept open for so long a period, enabled us to prevent the immense secretion from accumulating in the chest, in consequence of which the lungs were permitted to expand and the ribs to contract until the cavity had become obliterated. In the numberless introductions of the catheter, a bubble or two of air was the most ever allowed to enter the pleural cavity, and these at the withdrawal of the instrument. The temperature of the atmosphere seemed to exert a very decided effect upon the secretion; whenever there was a change in the weather, the air becoming cold, his symptoms were invariably aggravated; the pus became more copious, and the fœtor was increased.

This case, altogether, we consider a very remarkable one, for its violence, and for the speediness of the cure. From the commencement of his illness to the time of the first operation, only about thirty days elapsed, and from the day of the operation to the closing of the orifice, about four months transpired. During this time the instrument was introduced upwards of one hundred times, and we feel fully assured that not less than fifteen gallons of pus and serum were taken from the chest of the patient little sufferer.

We saw our patient to day, and find him in the enjoyment of good health. The heart is in its proper place, and respiration can be heard through the whole of the left lung. The side is still contracted, but no inconvenience is felt in consequence of the diminution of the lung. The cure may be considered perfect.

Frankfort, April 1, 1845.

The following is the letter referred to in the preceding article:

Owenton, Ky., July 10th, 1844.

Dr. Sneed:

Dear Sir—In reply to your letter inquiring for the particulars of my case, I hasten to reply, that I was taken with dysentery when about 13 or 14 years of age. The physicians gave me medicine which checked the disease suddenly and threw me into violent pain, which settled in my left side. I knew nothing for three days; from this time, or shortly afterwards, there was some swelling perceived in my left side, which gradually increased for sometime, when I was assured that matter had formed in the chest, but I refused to submit to an operation until I could see more appearance of it externally.

The pulsation of my heart was found in the right side, and Dr. Moore, of Shelbyville, and others, supposed that malformation of the chest existed, and that the heart had never occupied its proper situation. My side was finally opened, the incision into the chest being about one inch below the left nipple, and the opening through the skin about an inch below that. There was half a gallon of pus discharged before the opening was dressed, and I think as much more that night. During the time the pulsation of the heart moved several inches towards the left side.

In the course of about twelve months after this, a small tumor made its appearance two inches below the incision. This was also opened and discharged freely. About this time, or perhaps sooner, I began to expectorate a very offensive pus, often, in the morning, as soon as I began to move in bed, discharging a pint of so acrid a character that it seemed to excoriate the throat. Sometime after this, the communication became divided into the cavity of the chest. Every time that I breathed, the air would pass in and out at the opening with such violence that it could be distinctly heard, and its effects seen upon the discharge. When I coughed this sound could be heard across the room.

My physician now resorted to an injection, consisting of a decoction of barks—a part of which I usually coughed up, while a portion was discharged at the lower opening in my side, proving that the cavity of the sack was common to the two outlets, and that there was adhesion of the lungs to the side, and a direct communication between them and the external openings. After using the injection a few times I coughed up no more matter; the lower orifice in my side healed, and I gradually recovered. A probe for a long time could be passed five and a half inches directly towards the spine. I do not know that any medicine was of much advantage to me. My appetite was good all the time, which being indulged, supported the system and kept me from sinking. For a time dieting was attempted, but I grew worse every day, when the system was abandoned, and I was allowed bacon, beef-steak, and such nourishing articles. My side ran for two years, and I have no doubt that the discharge of matter from the two openings, and by coughing, amounted to sixty gallons. This statement may seem unreasonable, but I believe that my estimate is rather under than over the mark. Very little air now passes through the lobe of my left lung.

Yours, most respectfully,

F. REED.

REVIEWS.

Art. V.—*An Essay on the Philosophy of Medical Science.* By Elisha Bartlett, M.D., Professor of the Theory and Practice of Medicine, in the University of Maryland. Philadelphia: Lea & Blanchard. 1844. 8vo. pp. 312.

There are some things about which men may differ. There are some things to which they may attach too much importance. There are some things for which a man may endeavor to create an interest. But the healing art is not one of those things.

Men may differ as to the best form of government; or whether mankind would not be better off in a state of nature, as they suppose, without government. Jurists may differ, and do differ, as to the best civil code. But the time never will come when mankind will differ about the value of the healing art. Here even doctors do not differ. We may question in a general sense, we allow, whether more men are killed or cured by drugs; but yet, we repeat again, the time never will come, while man is in the state in which he now is, when the good physician will not be looked upon as the good christian; as a man above price, a light set upon a hill, which cannot be hid.

It is not as many suppose, that physicians of olden time were distinguished, and had their names handed down to us, because they were scholars, and were celebrated for their attainments in literature, and other accomplishments: not so. They immortalized themselves by healing the sick; by doing good. Physicians, in every age, have made an impression upon the world, and that impression will never be effaced.

From the cradle, nay, even from the womb to the grave, the doctor's services are felt and demanded.

Philanthropy applies more appropriately to the medical profession than to any other class of mankind.

Medical schools may multiply, the standard of the profession may be lowered, never so much; quacks may vend their nostrums with a diploma in their hands; but there stands the science, firm as the pillars of the earth; and her true disciple, the good physician, like the mariners compass, no discovery will ever supercede him.

Medicine is as truly a science as any other department of knowledge to which the term is applied. No science is more varied or extensive. The nature of this science, and in what it consists, we may dispute about. That a correct understanding of its principles is all important none will deny.

It is with these feelings and impressions that we undertake to review the work before us.

La Place, in the introduction to his Celestial Mechanics, expresses some concern, lest intelligent astronomers might have some difficulty in understanding his principles. So vague and indefinite have been the notions of medical men as to what constitutes medical science, and so unlike is this Essay to any work which has preceded it, that we fear, by a large majority of readers it will neither be appreciated nor understood.

Various are the reasons which will induce the different classes of medical men to decry this work. Already have we heard, and that too as a matter of congratulatory remark among some teachers of medicine, that the book fell still-born from the press.

All systematists will feel a deep interest in its rejection. The disciples of Cullen, Brown, Broussais, &c., will agree upon this, if upon nothing else. They will be unwilling to give up their theories, especially as the author of the new doctrine does not propose a theory of his own. *The classification and arrangements of the phenomena and their relationships,* will be to them a poor substitute for *inductive*

reasoning; and but a meagre definition of science and philosophy.

All medical science, says Dr. Bartlett, consists in ascertained facts, or phenomena, or events; with their relations to other facts, or phenomena, or events; the whole classified and arranged.

We read this proposition, with those which immediately follow it, to a physician of ordinary intelligence, who called at our office, and taking up the book, asked us to state to him, in a few words, in what Dr. Bratlett made the science of medicine to consist. He forthwith exclaimed, *there is no philosophy in that.* Such we candidly believe will be the judgment passed upon this work by a majority of its readers.

It is our deliberate opinion that the doctrine of our author will not be comprehended by a majority of physicians. They will be so unwilling to recieve it that they will not take the trouble to study it, so as to take hold of the true meaning of the author. For although no man could express his thoughts in a more clear and intelligible manner than the author has done, yet when the reader comes to inquire, what does this new doctrine amount to, if he feels the force of the reasoning, he will be so completely unsettled, that he will be like a boy lost in his father's forest: he may know the trees individually, but will be unable to find his way out of the woods.

The doctrine of Dr. Bartlett is, that medical science is to be considered as a branch of natural history, with separate and distinct parts: which parts are to be treated of and studied in themselves. That there are facts, or phenomena, or events, appertaining and belonging to each of these parts, nor can the one class of facts, or any of them be inferred or deduced by *a priori* reasoning, from the facts which are to be found in another, or other parts. That the science of medicine, as all natural science, is a science of observation of facts, or phenomena, or events, with their relationships to other facts, or phenomena, or events; the whole classified

and arranged. In other words, that the whole compass and power of the human mind, in the study of natural philosophy, medical science included, is limited to the observation of facts, phenomena, and events, and the classification and arrangement of them. That the mind cannot look beyond what, in a certain sense, it may be said to see. That an hypothesis or theory may lead to experiments, and thus be of some use, but that it can do no more. That the greatest man is the one who has observed (carefully and without prejudice, be it always understood) the largest number of facts, or phenomena, or events, in reference to any science or sciences, and who is the best able to classify and arrange them—to generalize them.

The genius of the minds, which have presided over American schools, and given direction to the young intellects of our new world, is not the genius of labor, of close, careful, untiring observation, of rigid generalization; but the genius of speculation.

Dr. Bartlett is of opinion that the view generally received and acted upon has given rise to a loose and vague philosophy, if we may honor the crude notions commonly taught with that name; that we have jumbled together incongruous things, as one of the old doctors did his drugs, expecting some good to come out of the mixture. Such, indeed, is the fact. So mixed and confused has been the study of medicine, that it is not only difficult to find a well educated physician who can give an intelligible summary of the most important divisions of medical science; but even among teachers we find that they have not so learned medicine. It is a very common complaint among students of medicine that the professors trespass upon the branches of each other; now the true reason for this is, that not one professor in a dozen could complete his course if he did not do so. It is in plain terms, ignorance of their own branch which makes them trespass on that of another; and yet they often say that they have not time to complete their course. The truth is that many of them would not know when it was comple-

ted. They have no definite idea of what is included in their department.

Bad as this state of things is, these gentlemen are not altogether to blame for the parts which they act. It is mainly attributable to the sort of philosophising which has prevailed, and to the manner in which medicine has been studied and taught.

Preliminary to the philosophy of medical science, we have in this Essay a condensed view of the principles of physical science. The first proposition is: "All physical science consists in ascertained facts, or phenomena, or events; with their relationships to other facts, or phenomena, or events; the whole classified and arranged: and in nothing else." The author says, "There seems to be a common feeling, that the facts, phenomena, and events, with their relationships, classified and arranged, constitute not the entire science to which they belong, but only the foundation of the science. There is a feeling, that these facts and relationships are to be used as elements, out of which the science is to be built up, or constructed, by what is called inductive reasoning. The feeling implies, and the avowed doctrine asserts, that the science is in this subsequent process of reasoning, and not in the facts themselves, and their relationships." To maintain this general doctrine, and to deny the correctness of the view commonly received is the burden of his labors. What the author says of physical science in the above quotation he insists upon as true respecting medical science, that it consists in ascertained facts, with their relations to other phenomena, classified and arranged. He continues:

"If it is true, even in physical science, as I have endeavored to show, that the doctrine stated in my first proposition, is only partially and imperfectly recognized, it is so to a much greater extent in regard to the same doctrine in its application to medical science; and the remarks already made upon this doctrine may be repeated with still less qualification, and with more emphatic significance, in connexion with my present subject. The fundamental and primary truth, that all medical science consists in the appreciable phenomena of

life, with their relationships, classified and arranged, and in nothing else, has never been generally admitted and received. This science, to a vastly greater extent than any other, has always suffered, and still continues to suffer, from the general prevalence of a spurious philosophy, and from vicious or imperfect methods of investigation; and one element in this false philosophy, leading to these mistaken methods, is to be found in the inadequate conception or half-belief of the doctrine above stated. Here, as in physical science with very few exceptions, men, claiming to be disciples of the Baconian philosophy, eloquent in their praises of what they call *inductive reasoning*, and full of earnest declamation against the dangers and the prevalence of false or premature generalizations, and of hypothetical speculation, have failed to see more than half the truth, and have, oftener than otherwise, fallen headlong into the errors which they were so ready to condemn. The feeling has been much more common in medical, than in physical science, that although facts and their relations might indeed, and must constitute the *foundation* of the science, the science still consisted in something more than these facts and relations;—that *upon* these latter the science itself was to be somehow built up, by that magical and creative process of the mind,—that evil genius of medical science,—called indeed, *induction*, but differing, when stripped of its disguises, in no single function or attribute, from that speculation, the place of which it professed, with promises as loud and pompous, as they have proved to be barren and empty, to occupy. The feeling has been, and still is,—as much, almost, since the time of Bacon as before,—that the science is in the *inductive or reasoning process, superadded to the facts and their relations*, more than in these latter themselves. Here, at the commencement of this part of my essay, I wish to enter my protest against this doctrine, in all its forms and modifications. I wish to show, that the science of medicine consists *in the phenomena of life, with their relationships, classified and arranged*,—WHOLLY, ENTIRELY, ABSOLUTELY. I wish to show, that these elements constitute,—not the foundation upon which, nor the materials, merely, with which, the science is to be subsequently constructed, by some recondite and logical process of the reason,—but that they *are* the science, and the whole science, already constructed, and so far completed; and that nothing can be superadded to them, by any act of the mind, which can in any way increase their value, or change their character.

The second proposition of our author maintains, that our knowledge of physiology is not deducible from our knowledge of anatomy. He asks the question, "Is there anything in the obvious physical properties of the glands of the human body,—is there any thing in their chemical composition, or in their minute molecular arrangement,—from which even the obscurest, and most shadowy glimpse could have been obtained of the several offices which they are destined to perform? Could the scalpel of the dissector, or the lenses of the optician, or the retort of the analyst, or all combined, have ever revealed to us the power of the liver to secrete bile, of the kidneys to secrete urine, of the mammary glands to secrete milk? The answers to the questions are in the negative—that the most intimate knowledge of the anatomy of an organ does not teach us the function of that organ."

But it does not follow that, because anatomy does not enable us to understand physiology, we can understand the latter without a knowledge of the former. There is, I conceive, some want of fairness in the order in which the subjects are arranged: namely, anatomy, physiology, pathology, therapeutics; although those which precede do not teach us those which follow, yet we cannot understand the subsequent without a knowledge of the antecedent.

We proceed a step further, says Dr. Bartlett. "Pathology is not founded upon physiology. The latter is not the basis of the former. The one does not flow from the other. Our knowledge of the one does not presuppose our knowledge of the other." The chapter from which this extract is made, is taken up chiefly in stating the qualifications of the doctrine just stated. The qualifications are, in our judgment, fundamental, and vast in their consequences. But our author is not of that opinion; and here, before stating the qualifications, we would remark, that Dr. Bartlett, though writing a book in opposition to all medical theory, is, nevertheless, decidedly under the influence of one himself. He is a disciple of the French school, and shews his theoretical bias more by oppo-

sing what is called humoralism, than by positively advocating solidism.

The first qualification to which we would call attention, is given by our author as follows: "I have already said that every simple and direct relationship is constant and invariable. Supposing now the physiological actions, relationships of the body, to be fully ascertained, we may safely conclude, independent of positive experience, that a change in those relationships will be followed by a change in the actions themselves, and, and in the results of those actions. Thus after physiology has taught us, as far as it can teach us, the action of the oxygen of the atmosphere upon the blood, we may safely and positively conclude, prior to all experience, and independent of it, that if this action is interrupted, all the subsequent physiological processes, with which it is connected will be also, and necessarily more or less disturbed; and the same thing is true of physiological actions and relationships."

The author thus expresses himself on the same point:

"Finally, so intimate, and complicated, and manifold, are the physiological relationships of the living economy; so closely is each part connected with the rest; so readily and powerfully do these parts act and react upon each other; so complete, in many instances, is the union and the coöperation of the mechanical, the chemical, and the vital processes, that, independent of actual experience, we might safely conclude, that an injury inflicted upon one part of the body might often affect, more or less seriously, many other parts; and that a disturbance, or suspension. of any one of the three great processes might, in many cases at any rate, disturb, or suspend, the other two. Having ascertained, by observation, the complicated physiology of the circulation and of respiration; having ascertained the existence of various mechanical, chemical, and vital actions, and their necessary coöperation in order to produce a certain result, consisting in the circulation, the oxygenation, and the decarbonization of the blood; it would follow, as a matter of course, and independent of positive experience, that a disturbance, or suspension of one of these associated actions, should disturb, or suspend, the others. Inasmuch as the integrity of the mechanical contri-

vances for the repeated exposure of the blood to the influence of the atmospheric oxygen is necessary, to secure this exposure, and inasmuch as this exposure is necessary, in order that the oxygenation, and decarbonization of the blood should be effected; and inasmuch as this change in the blood is essential in order to prepare it for answering its purposes in the vital processes of the body, it follows, necessarily, that any disturbance, or imperfection, in the first mechanical process, will be followed by corresponding disturbances and imperfections in the subsequent and associated chemical and vital processes. In the same way, also, having ascertained that certain substances are eliminated from the body by the physiological actions of the liver, and the kidneys, we might justly come to the conclusion, without waiting for the positive teachings of experience, that the retention of these substances within the system would be followed by unfavorable results."

We would ask, do not these qualifications cover the entire ground of functional disorder? Do they not apply with great force to the causes and nature of miasmatic fevers? Do they not apply to scrofula, since Baudelocque has so conclusively shewn that it originates in the breathing for a length of time of bad air, or air intermixed with carbonic acid or other gases? But the author takes a very different view of them. In justice to his profound and extensive knowledge of his subject he was compelled to make them. But will not every reader be astonished to hear him say—"they are more nominal than real. When closely examined and analysed they reduce themselves within very insignificant dimensions."

This multiplication of his own qualifications, which we maintain are fundamental, philosophical, and true, proves our author to be himself a theorist, and that he attaches undue importance to lesions of structure.

If it is granted as a fact, that from a particular cause, miasm for instance, the blood has been altered in its composition, we can as readily comprehend how certain organs must become disordered, as we could in the first instance understand how the blood became changed. It is not a specultion to say that the liver will be implicated under the continued opera-

tion of such a cause; and that local congestions, as of the head, lungs, and abdominal viscera, may be anticipated.

The departure from the healthy, natural action of vital organs is all that we have to guide us in many cases of the worst form of disease. We may know that a particular lesion exists, but it profits us nothing. We cannot reach the local affection until we remove the difficulty in the way of vital functions; and the means by which this is to be accomplished have no reference to the lesion whatever.

That our author belongs to the class of solidists, we have only to refer to his chapters on intermittent, remittent, and yellow fevers, in his work on typhus and typhoid fever, to be convinced. In his remarks on these fevers we have clear evidence of his leaning to this particular school. Else why should he refer to the writings of men who have seen scarcely anything of the diseases under consideration, when there are so many volumes abounding in facts observed by men who lived for years within the tropics, or near them; by men of the most thorough medical education, and remarkable for the patience and carefulness of their observations; men who tested their theories by years of laborious and successful practice? Why should he refer to Trousseau, Louis, Gerhard, and Stewardson, and pass by Bancroft, Pringle, Lind, Cleghorn, Mackintosh, and James Johnson, not to mention others of equal celebrity? Simply, as we conceive, because the gentlemen to whom he is pleased to refer, with Louis at their head, advocate the system which he adopts; and because they have devoted more time to the examination of anatomical lesions.

It would be difficult to find a more remarkable instance, than is furnished by our learned and ingenious author, of the impossibility of writing on medicine without becoming the advocate of some theory or system. Evidently his is a mind thoroughly imbued with the true inductive philosophy; his conceptions of what constitutes science are eminently just, and yet while repudiating all hypothesis he gives indu-

bitable proof of being under the dominion of a mere hypothesis. His work might be called an essay on the architecture of medicine. It places in bold relief the different columns and pillars; he has wrought out each part in exact and due proportions; they are finished in a masterly manner. But the architect is unwilling that the edifice should be constructed. He must have each part stand separate and alone; and although he sees that the parts are made to fit into each other, yet he declares that this is only in appearance, and not a reality—that they are in fact isolated, and so they must stand.

We agree with Dr. Bartlett, that anatomy, physiology, pathology, and therapeutics are separate and distinct; and that to suppose that a knowledge of one will give us a knowledge of the other is a great mistake,—one which cannot be corrected too soon. We believe that a vast amount of false philosophising has prevailed and still prevails on these subjects; we believe that the geologist who first discovered and made known the arrangement of the strata of the earth into primary, secondary, and tertiary rocks, with their several subdivisions, scarcely did more for the student of geology than our author has done in this work for the student of medicine; and had he been free from the bias of the French school he would have made his essay far more valuable than it is. But he had adopted in theory, and acted upon in practice, M. Louis's method, long before he wrote a page of his work; and we cannot resist the belief that his object was not more to correct errors which really exist, than to lay the foundation for M. Louis's system.

If pathology consists in the facts, or phenomena, or events of disordered action, with the relationships of these facts classified and arranged, as in fact it does, may we not say that there are relationships existing between these facts, and others observed in the domain of physiology? Should not the physician at the bed-side of a patient endeavor to ascertain every fact, whether it be found in one or the other departments of the science, and then consider its relationships? Is it contrary to sound philosophy to take a comprehensive view

of all together? We think not, and we are obliged therefore to deprecate this unreasonable disposition to build up a wall of separation between the different branches of the science.

Man is a living being, and in the same body we find, so far at least as the solid organism is concerned, diseased and sound parts. We shall never be able to draw the line of separation between the sound and the unsound as we can between a stratum of primary and of secondary rock; nor can we weigh the quantum of health and disease as we can the chemical elements of a block of marble.

Dr. Bartlett thinks that diseases may be classified and arranged as plants are, according to the natural system of botany. We greatly doubt this. We incline too much to the humoral doctrine to allow the practicability of any such thing.

Our author is unable to believe that our facts are sufficient to prove that malaria is the cause of intermittent, remittent, and yellow fevers. Now it strikes us that the reason for this remarkable scepticism is to be found in the fact, that almost all who hold to the miasmatic origin of the fevers in question, believe that it enters the circulation through the lungs along with the air we breathe. This view involves so many humoral consequences, which are inevitable, that to avoid them, Dr. Bartlett boldly denies the first position—that miasmata are the cause of these fevers.

Dr. Bartlett seems to think that because the successful practice of medicine has not kept pace with the improvements and discoveries which have been made in some of the branches of the science, we have corroborative proof of his position, that a knowledge of the one branch does not teach us a knowledge of the other. He refers to Mr. Jefferson's opinion concerning the practice of medicine before and after the discovery of Harvey. If medicine was practiced as successfully before the discovery of the circulation of the blood as it has been since, the fact is in opposition to the doctrine of our author. We believe that many men have been more dis-

tinguished as successful practitioners than is M. Louis at this day. Indeed Dr. Bartlett admits himself that English physicians are more successful as practitioners than the French.

Before pathological anatomy came to be thought so very necessary a part of medical education, and the doctrine of lesions became the order of the day, physicians made out their diagnosis more by a comparison of disordered with healthy actions, than in any other manner. They did not go farther perhaps than Dr. Bartlett allows us to go in his qualifications to his general doctrine, but that was far enough; and upon such principles they practised successfully; more so, perhaps, than Louis, or Chomel. To shew how uncertain are pathological appearances after death we may quote M. Louis's opinion of Broussais. He holds that Broussais, with all his knowledge and extensive observation, was not qualified to pronounce upon the cause of death after a post mortem examination. Broussais was a "theorist;" and so may be Louis; and so may be our author.

When Dr. Bartlett says that therapeutices is not founded upon phathology, he merely intends to say, as we conceive, that our knowledge of a particular lesion does not enable us, *a priori*, to determine upon the remedy. We do not understand him to mean, that after a remedy has been discovered, there may not appear to be a relationship between the disease and the remedy. We will give an illustration.

When Champollion first directed his attention to the Egyptian hieroglyphics he could not conceive of their import; but after the Rosetta stone gave him a clue, he went on gradually to read, until after a time, he was prepared to relate to the world the mysteries of the pyramids. So in the science of medicine. If our remedies act in accordance with the laws of the animal economy at all, there must be a relationship between the diseased part and the remedy; accident may discover it at first; but after it has been discovered, it is not unreasonable to suppose that the mind may perceive that relationship. When we shall come to understand diseases more clearly, when we shall have more carefully investigated the

action of remedies, and learned better to discriminate between the physiological and therapeutical action of a medicine, we shall be able to perceive these relationships more clearly, and no doubt discover a greater number of them.

We have been reluctant to differ with our excellent and distinguished author on these points, but notwithstanding the errors into which we are constrained to believe he has fallen, our estimate of his work is such, that we cordially recommend it to every young physician as a most useful aid in the prosecution of medical study. We believe the award of posterity concerning it will be favorable; and for ourselves, we are agreed that the medical philosophy of our century shall be judged of by it. J. C. D.

Lexington, April 21st, 1845.

ART. VI.—*A Treatise on Poisons in relation to Medical Jurisprudence, Physiology, and the Practice of Physic.* By ROBERT CHRISTISON, M. D., F. R. S. E.; Professor of Materia Medica in the University of Edinburgh; Fellow of the Royal College of Physicians, &c.; Member of the American Philosophical Society, of the Royal Academy of Medicine of Paris, &c., &c., &c. First American, from the Fourth Edinburgh Edition. Philadelphia: Ed. Barrington & Geo. D. Haswell. 1845. 8vo. pp. 756.

The subscribers of the Select Medical Library and Bulletin of Medical Science are already in possession of this elaborate and admirable treatise, the editor, with his usual judgment, having selected it for the beginning of a new year. It is a work the want of which has long been felt in this country, and appearing now in an unexpensive form it may be expected in due season to take its place in the libraries of all reading physicians. No one need hesitate about its purchase on account of any apprehension that it may be superceded in any reasonable time by a more perfect work, since

it would be difficult to point out in it any deficiency, or to show how in any particular it might be improved. For the student seeking to store his mind with all that relates to the subject of poisons, and equally for the practitioner who may have occasion to refresh his memory concerning the tests and antidotes of these agents, it is safe to say that it is one of the most finished and satisfactory guides that any language could furnish. Higher authority than that of Christison could not easily be found on any point of the profession towards which he has directed his mind, but having made poisons a matter of special study for many years, during which he has enjoyed ample opportunities for becoming familiar with all the details of the subject, a work abounding in original observations and opinions, exhibiting the science of toxicology in its present advanced state, and entitled, therefore, to take the first rank in its class, is what might have been looked for from his pen. We congratulate our readers on the opportunity now afforded them of procuring such a treatise.

The work is divided into two parts, the first treating of general poisoning, and the second of individual poisons. Under the first head the physiological action of poisons is discussed. This may be either local or remote. The local effects of poisons are various, some acting chemically and corroding the parts to which they are applied, others producing irritation or inflammation, and a few making a peculiar impression on the sentient extremities of the nerves, unattended by any manifest change of structure. The alkalis and concentrated mineral acids act in the manner first mentioned; alcohol, cantharides, and tartar emetic inflame or irritate the skin and other tissues to which they are directly applied; arsenic, euphorbium, and the diluted mineral acids affect the stomach in the same way. Of the third mode, by nervous impression, we have fewer unequivocal instances, but the impression of the aconitum napellus, or monkshood, upon the tongue and lips, when chewed, a sense of numbness and tingling, lasting for some hours, and unconnected with any affection of the general nervous system, is considered an example, as is also the

numbness of the end of the finger from contact with strong hydrocyanic acid, a numbness as from the local action of intense cold. It is seldom that death results from the local action of a poison, whether corrosive, inflammatory, or nervous; but the effects are radiated to distant organs, the suspension of the function of which is the cause of the fatal termination. Their remote action, therefore, is far the most important.

We have an example, and a very striking one, of this species of action in oxalic acid. It is corrosive, and cases are recorded in which it has produced perforations in the stomach; yet it never kills by destroying the function of that organ, for in some instances no trace of injury in the stomach could be found when death had followed a large dose of the poison. This acid has been known to kill a man in ten minutes, and a dog in as short a time as three minutes, but in such instances it was considerably diluted, which circumstance favored the remote action of the poison while it diminished, in the same proportion, its local action. Let us look for a moment at the way in which the influence of a poison is conveyed from one organ to another.

It is plain that there are but two ways in which this can be accomplished; the poison must be absorbed, or must act through the medium of the nerves upon distant organs. In the infancy of toxicology all poisons were believed to act in the latter way; but at present the other doctrine is more prevalent, while a recent theory, combining both views, represents that, although many poisons do enter the blood, their operation nevertheless is directly upon the nerves of the blood-vessels, and thus remotely through them upon the brain or other organs.

In the case of poisons that manifestly injure the structure of the organ to which they are applied, the affection of remote organs is undoubtedly sympathetic. Such is the fact with regard to the mineral acids, in which dilution lessens and even takes away altogether their remote poisonous action. But it is by no means so clear that distant organs sym-

pathise with the peculiar local impressions, styled nervous, which are unattended by any visible derangement of structure. It is in relation to this that the chief disputes among physiologists have arisen, and we agree with our author that the existence of this variety of action by sympathy is doubtful. At one time it was pretty generally received as the true explanation of the mode in which many poisons affected the system at large, and facts such as the following were thought to be inexplicable upon any other principle:

"Magendie, speaking of the pure hydrocyanic acid, compares it in point of swiftness of action to the cannon ball or thunderbolt. In the course of certain experiments made not long ago with the diluted acid by Dr. Freer, Mr. Macauly and others, to decide the true rapidity of this poison, several dogs were brought under its influence in ten, eight, five, and even three seconds; during an experimental inquiry I afterwards undertook for the same purpose, I remarked on one occasion that a rabbit was killed outright in four seconds; and Mr. Taylor has more recently stated, that he has seen the effects induced so quickly in cats, that there was no sensible interval of time between the application of the poison to the tongue and the first signs of poisoning. Strychnia, the active principle of nux-vomica, acts sometimes with a speed little inferior to that of hydrocyanic acid; for Pelletier and Caventou have seen its effects begin in fifteen seconds. Alcohol, according to Sir B. Brodie, also acts on animals with equal celerity; for when he intoduced it into the stomach of a rabbit, its effects began when the injection was hardly completed."

But, as remarked by Dr. Christison, the progress of physiological discovery has lately brought the soundness of these views into question.

"Some years ago Dr. Hering of Stuttgardt showed that the round of the circulation may be accomplished by the blood much more speedily than had been conceived before; for the ferrocyanide of potassium, injected into the jugular vein of a horse, was discovered by him throughout the venous system at large in the short space of twenty or thirty seconds, and consequently must have passed in that period throughout the whole double circle of the pulmonary and sys-

temic circulation. This discovery at once shook the validity of many, though not all, of the facts which had been previously referred to the agency of nervous transmission on the ground of the celerity with which the effects of poisons are manifested. More recently an attempt has been made by Mr. Blake to prove, that the circulation is so rapid as to admit even of the swiftest cases of poisoning being referred to the agency of absorption. Mr. Blake, who is altogether opposed to the occurrence of nervous transmission in the instance of any poison, has found that ammonia, injected into the jugular vein of a dog, was indicated in its breath in four seconds; and that chloride of barium or nitrate of baryta, introduced into the same vessel, could be detected in the blood of the carotid artery in about sixteen seconds in the horse, in less than seven seconds in the dog, in six seconds in the fowl, and in four seconds in the rabbit. These interesting discoveries, however, will not absolutely destroy the conclusiveness of all the facts quoted above in support of the existence of a sympathetic action."

No such difficulties attend the other doctrine. The action of poisons through the medium of the circulation is established by conclusive evidence, a portion of which our author thus sums up:

"First, they disappear during life from the shut cavities or other situations into which they have been introduced; that is, they are absorbed. Several clear examples to this effect have been related by Dr. Coindet and myself in our paper on oxalic acid. In one experiment four ounces of a solution of oxalic acid were injected into the peritoneal sac of a cat, and killed it in fourteen minutes; yet, on opening the animal, although none of the fluid had escaped by the wound, we found scarcely a drachm remaining. In recent times Professor Orfila has proved that various poisons, such as arsenic, tartar-emetic, and acetate of lead, disappear in part or wholly from wounds into which they had been introduced. Next, many poisons act with unimpaired rapidity, when the nerves supplying the part to which they are applied have been previously divided, or even when the part is attached to the body by arteries and veins only. Dr. Monro, *Secundus*, proved this in regard to opium; and the same fact has been since extended by Sir B. Brodie and Professor Emmert to wourali, by Magendie to nux vomica, by Coullon to hydrocyanic acid, by

Charret to opium, and by Dr. Coindet and myself to diluted oxalic acid. Magendie's experiment was the most precise of all: for, besides the communication with the poisoned part being kept up by a vein and an artery only, these vessels were also severed and reconnected by two quills. Farther, many poisons will not act when they are applied to a part of which the circulation has been arrested, even although all its other connections with the body have been left entire. This has been shown distinctly by Emmert in regard to the hydrocyanic acid; which, when introduced into the hind-leg of an animal after the abdominal aorta has been tied, produces no effect till the ligature be removed, but then acts with rapidity. An experiment of a similar nature performed by Mr. Blake with the wourali poison yielded the same result. Again, many poisons act with a force proportional to the absorbing power of the texture with which they are placed in contact. This is the criterion which has been commonly resorted to for discovering whether a poison acts through the medium of the blood. It is applicable, however, only when the poison acts sensibly in small doses; for those which act but in large doses cannot be applied in the same space of time over equal surfaces of different textures. The difference in the absorbing power of the different tissues has been well ascertained in respect to a few of them only. The most rapid channel of absorption is by a wound, or by immediate injection into a vein; the surface of the serous membranes is a less rapid medium, and the mucous membrane of the alimentary canal is still less rapid. Now it is proved of many poisons that, when applied in similar circumstances to these several parts or tissues, their activity is proportional to the order now laid down. Lastly, it has been proved of nux-vomica, that if the extract be thrust into the paw of an animal after a ligature has been tightened round the leg so as to stop the venous, but not the arterial circulation of the limb, blood drawn from an orifice in a vein between the wound and the ligature, and transfused into the vein of another animal, will excite in the latter the usual effects of the poison, so as even to cause death; while, on the contrary, the animal from which the blood has been taken will not be affected at all, if a sufficient quantity be withdrawn before the removal of the ligature. These interesting facts, which are capable of important practical applications, were ascertained by M. Verniere."

Still a question remains as to the action of the poisons after they have gained admission into the blood. It is not yet

perfectly clear that they are transmitted in substance to the organs affected by them. A different theoretical view has been taken of the facts by Dr. Addison and Mr. Morgan, and as it is ingenious we give it in the words of our author. Their theory is—

"That the action may really take place by the poison producing on the sentient extremities of the nerves of the inner membrane of the blood-vessels a peculiar impression which is conveyed through the nerves to the part ultimately affected. They have endeavored to found this theory upon evidence, that the poison is not carried beyond the venous system; or that, if conveyed farther, it is carried incidentally, and not for the purpose of impregnating the textures of the organ which suffers. The evidence they have brought forward on this head is chiefly the following. 1. Poisons which act on a particular organ at a distance do not act more quickly when introduced into the artery which supplies it, than when introduced into its vein, or even into the principal artery of a distant part of the body. 2. If a poison be introduced into a great vein with a provision for preventing its passage towards the heart, it will act with as great rapidity, as if no obstacle of the kind existed. Thus, if the jugular vein, secured by two temporary ligatures, be divided between them and reconnected by a tube containing wourali, the animal will not be affected more quickly on the removal of both ligatures, than on removing only the ligature farthest from the heart. 3. The arterial blood of a poisoned animal is incapable of affecting another animal. Thus, if the carotid artery and jugular vein of one dog be divided, and both ends of each reciprocally connected by tubes with the divided ends of the corresponding vessels of another dog, and extract of nux-vomica be introduced into a wound in the face of one of them,—the animal directly poisoned alone perishes. and the other remains unharmed to the last."

Poisons vary as to their remote action, something like an elective affinity being manifested by certain articles for particular organs. The heart, for example, is specially affected by tobacco and the upas antiar; the pulmonary capillaries by tobacco and tartar emetic; the brain by narcotics generally; the spinal marrow by conia and nux vomica; various glands by mercury; and the alimentary canal by arsenic. It is fur-

ther stated by writers on this subject that chromate of potash excites inflammation of the conjunctiva, and that ergot has been known to produce gangrene in different parts of the body, with dropping off of the toes.

The effects of poisons are modified by various circumstances, among which may be mentioned quantity, the state of aggregation, the chemical combination, mixture, difference of tissue, habit and idiosyncrasy, and diseased states of the body. Thus, arsenic in a large dose may prove speedily fatal, while in smaller quantities it may induce inflammation of the alimentary canal and a lingering death. White hellbore in minute quantities excites gastritis, and in larger doses brings on giddiness, convulsions and coma. The state of aggregation as a modifying circumstance is easily appreciated; poisons must be in solution, or at least be soluble, to exert their peculiar effects. The effect of chemical combination is seen in the case of all antidotes, in which poisons are rendered inert by their union with other bodies which themselves may be poisonous. The concentrated mineral acids and thecaustic alkalies are examples, both poisonous, but becoming harmless by combining. Mixture may increase the activity of a poison if thereby it is rendered more capable of reaching the circulation, but where the mixture is with the food in the stomach, as when taken during a meal, poisons are rendered less injurious by this circumstance. Arsenic is often ejected in such cases when it would be fatal on an empty stomach. Difference of tissue in the case of poisons which enter the circulation is a modifying circumstance, but where the poison exerts a corrosive influence the effect is nearly the same to what part soever applied. Carbonic acid is harmless in the stomach, but most deleterious in the lungs. The venom of the viper may also be swallowed with impunity, but if injected into a vein excites a train of alarming symptoms. Oxalic acid if not so concentrated as to produce irritation does not affect the stomach, and only becomes deleterious when it has passed from the digestive organs into the blood. Poisons which owe their power to absorption are most active when

introduced into a recent wound; next to this in contact with the serous tissues; then in the mucous passages; and least of all when applied to the skin. Habit and idiosyncrasy are to be taken into the account, as many cases on record, and the observation of every one must make evident. Mercury is poisonous in the smallest doses to some constitutions; ipecacuan brings on a distressing asthma in some; tartar emetic is followed by alarming symptoms in others; while, as to the effect of habit, we see persons daily indulging in potations of ardent spirits, and others swallowing quantities of opium. which would be fatal to the uninitiated.

Our author makes an application of these facts to practice. thus;

"As an appendix to what has been said respecting the physiological action of poisons, and the causes by which it is liable to be modified, I shall here state shortly certain applications to the treatment of poisoning.

In the instance of internal poisoning, the great object of the physician is to administer an antidote or counter-poison. Antidotes are of two kinds. One kind takes away the deleterious qualities of the poison before it comes within its sphere of action, by altering its chemical nature. The other controls the poisonous action after it has begun, by exciting a contrary action in the system. In the early ages of medicine almost all antidotes were believed to be of the latter description, but in fact very few antidotes of the kind are known.

Chemical antidotes operate in several ways, according to the mode of action of the poison for which they are given. If the poison is a pure corrosive, such as a mineral acid, it will be sufficient that the antidote destroy its corrosive quality: Thus the addition of an alkali or earth will neutralize sulphuric acid, and destroy or at least prodigiously lessen its poisonous properties. In applying this rule care must be taken to choose an antidote which is either inert in itself, or if poisonous, is like the poison for which it is given, a pure corrosive or local irritant, and one whose properties are reciprocally neutralized.

If the poison, on the other hand, besides possessing a local action, likewise acts remotely through absorption, or by an impression on the inner coat of the vessels, mere neutralization of its chemical properties is not sufficient; for we have seen above that such poisons act throughout all their chemi-

cal combinations which are soluble. Here, therefore, it is necessary that the chemical antidote render the poison insoluble or nearly so; and insoluble not only in water, but likewise in the animal fluids, more particularly the juices of the stomach. The same quality is desirable even in the antidotes for the pure corrosives; for it often happens that in their soluble combinations these substances retain some irritating, though not any corrosive power. When we try by the foregoing criterions many of the antidotes which have been proposed for various poisons, they will be found defective; and precise experiments have in recent times actually proved them to be so.

The other kind of antidote operates not by altering the form of the poison, but by exciting in the system an action contrary to that established by the poison. On considering attentively, however, the phenomena of the action of individual poisons, it will be found exceedingly difficult to say what is the essence of a contrary action, and still more how that counteraction is to be brought about. Accordingly, few antidotes of the kind are known. Physiology or experience has not yet brought to light any mode of inducing an action counter to that caused by arsenic and most of the irritant class of poisons. It appears probable that the remote operation of lead may be sometimes corrected by mercury given to salivation, and that the violent salivation caused by mercury may be occasionally corrected by nauseating doses of antimony. But these are the only instances which occur to me at present of antidotes for irritant poisoning which operate by counteraction, unless we choose to designate by the name of antidote the conjunction of remedial means which constitute the antiphlogistic method of cure. In the class of narcotics we are acquainted with equally few constitutional antidotes, although the nature of the action of these poisons seems better to admit of them. Ammonia is to a certain extent an antidote for hydrocyanic acid, but by no means so powerful as some persons believe; and I am not sure that in this class of poisons we can with any propriety mention another antidote of the constitutional kind.

On the whole, then, it is chiefly among the changes induced by chemical affinities that the practitioner must look for counter-poisons; and the ingenuity of the toxicologists has thence supplied the materia medica with many of singular efficacy. When given in time, magnesia or chalk is an antidote for the mineral acids and oxalic acid, albumen for corrosive sublimate and verdigris, bark for tarter-emetic, common salt for lunar caustic, sulphate of soda or magnesia for sugar of lead

and muriate of baryta, chloride of lime or soda for liver of sulphur, vinegar or oil for the fixed alkalis; and these substances act either by neutralizing the corrosive power of the poison, or by forming with it an insoluble compound.

"In recent times a new object in the treatment of poisoning has been pointed out by the discoveries made in its physiology. As it has been proved that many of the most deadly poisons enter the blood, and in all probability act by circulating with that fluid, so it has been inferred that an important object in the treatment is to promote their discharge by the natural secretions. In support of this reasonable inference it has been lately rendered probable by Orfila, as will be seen under the head of the treatment of the effects of arsenic, that it is of great advantage in some forms of poisoning to increase the discharge of urine.

"In the instance of external poisoning the main object of the treatment is to prevent the poison from entering the blood, or to remove it from the local vessels which it has entered.

"One mode, which has been known to the profession from early times, and after being long in disuse was lately revived by Sir D. Barry, and applied with success to man, is the application of cupping glasses to the part where the poison has been introduced. This method may act in various ways. It certainly prevents the farther absorption of the poison by suspending for a time the absorbing power of the vessels of the part covered by the cup. It also sucks the blood out of the wound, and may consequently wash the poison away with it. Possibly it likewise compresses the nerves around, and prevents the impression made by the poison on their sentient extremities from being transmitted along their filaments.

"Another mode is by the application of a ligature between the injured part and the trunk, so as to check the circulation. This is a very ancient practice in the case of poisoned wounds, and is known even to savages. But as usually practised it is only a temporary cure; as soon as the ligature is removed the effects of the poison begin. It may be employed, however, for many kinds of poisoning through wounds, so as to effect a radical cure. We have seen that most poisons of the organic kingdom are in no long time either thrown off by the system or decomposed in the blood. Hence if the quantity given has not been too large, recovery will take place. Now, by means of a ligature, which is removed for a short time at moderately distant intervals, a poison which has been introduced into a wound beyond the reach of ex-

traction, may be gradually admitted into the system in successive quantities, each too small to cause death or serious mischief, and be thus in the end entirely removed and destroyed. Such is the practical application which may be made of some ingenious experiments performed not long ago by M. Bouillaud with strychnia, the poisonous principle of nux-vomica.

"The last mode to be mentioned is by the combination of the ligature with venesection, deduced by M. Verniere from his experimental researches formerly noticed. Suppose a fatal dose of extract of nux-vomica has been thrust into the paw of a dog; M. Verniere applies a tight ligature round the limb, next injects slowly as much warm water into the jugular vein as the animal can safely bear, and then slackens the ligature. The state of venous *plèthora* thus induced completely suspends absorption. The ligature is next tied so as to compress the veins without compressing the arteries of the limb, and a vein is opened between the wound and the ligature in such a situation, that the blood which flows out must previously pass through, or at least near the poisoned wound. When a moderate quantity has been withdrawn, the ligature may be removed with safety; and the extraction of the poison may be farther proved by the blood that has been drawn being injected into the veins of another animal; for rapid death by tetanus will be the result. It is not improbable that in this plan the preliminary production of venous plethora may be dispensed with; and then the treatment may be easily and safely applied to the human subject."

The evidence of general poisoning is examined by Dr. Christison with great ability. The circumstances to be attended to are, as to the symptoms, their suddenness and rapidity, their regularity of increase, uniformity, beginning soon after a meal, attacking the individual in a state of health; of which it may be said, that no single one is of itself conclusive, but that a number concurring a strong suspicion of poisoning might arise, especially if there were at the same time moral proof of the crime. The same circumstances, or the absence of them, in other cases may dispel any unfounded suspicions of poisoning. We give the following interesting case, cited by our author, as in point:

"The Crown Prince of Sweden while in the act of reviewing a body of troops on the 28th May, 1810, was observed suddenly to waver on his horse; and soon afterwards he fell off while at the gallop, was immediately found insensible by his staff, and expired in half an hour. As he was much beloved by the whole nation, a rumour arose that he had been poisoned; and the report took such firm root in the minds of all ranks, that a party of military, while escorting the body to Stockholm, were attacked near the city by the populace, and their commander, Marshal Fersen, murdered; and Dr. Rossi, the prince's physician, after narrowly escaping the same fate, was in the end obliged to quit his native country. Now, no other poison but one of the most active narcotics could have caused such symptoms, and none of them could have proved so quickly fatal unless given in a large dose. It was proved, however, that on the day of his death the prince had not taken any thing after he had breakfasted; and an interval of nearly four hours elapsed after that till he fell from his horse. This fact alone, independently of the marks of apoplexy found in the head after death, and the warning symptoms he had repeatedly had, was quite enough to show that he could not have died of poison, as it was incompatible with the known action of the only poisons which could cause the symptoms. This is very properly one of the arguments used by the Medical Faculty of Stockholm, which was consulted on the occasion."

In addition to these evidences we are to inquire into the morbid appearances found in the body of the deceased, and finally to draw all the information that can be afforded by chemical analysis. We must determine first what the poison is, and subsequently whether it was administered during the life of the individual, or inserted into some part of his body after death. It is to be searched for in the stomach, the texture of the liver, in the blood, the urine, or in the matter ejected from the stomach, or known to have been partaken of by the victim as food or drink.

Some years ago a steam boat was ascending the Ohio towards Pittsburgh, with passengers as usual from various parts of the country. The subject of abolition happened to be discussed, and Southern gentlemen spoke their opinions freely in the hearing of a black waiter or cook. That night short-

ly after supper, a number of the passengers were seized with nausea, vomiting, and other symptoms of poisoning, which in the case of one or two persons were so violent as to create much alarm; they, however, all recovered. On investigation it appeared that those only were affected who had taken corn bread at supper. A suspicion was hence excited that the black man, apprised of the fact that southern people are apt to eat corn bread, had mixed arsenic with it. He was charged with the crime but made his escape by jumping into the river. Some of the bread was sent to us for analysis, and was carefully examined in the Laboratory of the Medical Institute. Besides answering to all the tests for arsenic in the most conclusive manner, it gave curious collateral evidence of containing a poison in killing all the flies about the room, which were attracted to the water in which we boiled the bread.

Although poison may not be detected in the alimentary canal, or the organs, or in any of the articles of food, moral evidence of poisoning sometimes brought out, may establish the charge. Christison relates the case which follows as being of that character:

"A man of doubtful character and morals, well acquainted with chemistry and medical jurisprudence, and of disordered finances, was known to harbour a design on a friend's wife, who possessed a considerable fortune. At last he one morning invited the husband to breakfast with him at a tavern; and they breakfasted, in a private apartment, on beef-steaks, fried potatoes, eels, claret, and rum. They had scarcely commenced the meal when his guest complained of feeling unwell; and soon afterwards he vomited violently. This symptom continued, along with excruciating pain in the belly, for a long time before the prisoner sent for medical aid; indeed he did not procure a physician till the sufferer had been also attacked with very frequent involuntary purging. The physician, who, before seeing his patient, had received the prisoners explanation of the apparent cause of the illness, was led at first to impute the whole to cholera caught by exposure to cold; but on returning at seven in the evening and finding the gentleman had been dead for an hour, he at once exclaimed that

he had been poisoned. On the body being inspected much external lividity was found, contraction of the fingers, and great inflammation, of the stomach and intestines, presenting an appearance like that of gangrene. On analyzing some fluid left in the stomach, no arsenic or other poison could be detected. The attention of the inspectors was turned specially to arsenic, because the prisoner was proved to have bought that poison, and to have made a solution of some white powder in his kitchen not long before the deceased died. The prisoner in his defence stated, that the deceased had been for some time much weakened by the use of mercury, and while in this state was seized with cholera; and he likewise attempted to make it probable that the man, in despair at his not recovering from a venereal disease, might have committed suicide. The council of physicians who were required to give their opinion on the case state on the contrary, that the deceased was a healthy man, without any apparent disposition to disease; that there was no pretext whatever for supposing suicide; that the inflammatory state of the stomach and bowels supplied strong probability of poisoning with arsenic, but not certain evidence; that acute gastritis from natural causes is always attended with constipation; that the deceased presented symptoms of stupor and other signs of derangement of the nervous system remarked in rapid cases of poisoning with arsenic; that cholera is very rare at the end of November, the season when this incident occurred; and that the poison might well be discharged by vomiting. Although all the prisoner's statements in defence were contradicted by satisfactory proof, and the medical evidence of poisoning was supported by a chain of the strongest general circumstances, the crime was considered by the court as not fully proved, because the prisoner could not be induced to confess, and because poison was not actually detected in the body. But on account of the very strong probability of his guilt, he was, in conformity with the strange practice of German courts in the like cases, condemned to fifteen years' imprisonment. In this instance—considering the kind of symptoms, their commencement during a meal, the rapidity of death, the signs of violent inflammation in the stomach after so short an illness, and the facility with which the absence of poison in the contents of the stomach may be accounted for, more especially if it be supposed that the poison was administered in solution,—I consider the medical evidence of death by poisoning so very strong, that, the general evidence being also extremely strong, the prisoner's guilt was fully demonstrated.

The moral evidence is of the greatest importance in questions of the sort. Thus, a woman was seized early in the morning with symptoms indicating that she had taken an acrid poison. A farmer, who had some knowledge of medicine, being called to see her, learned that she had breakfasted on porridge made by her husband of corn meal, and on analysis the meal proved to contain arsenic. Again; a woman was found who had evidently been poisoned with nitric acid; but whether she had taken it to commit suicide, or had received it from another was to be determined by circumstances. Stains were discovered on her face as if the acid had been spilled there in the act of forcing it down her throat, and on further examination similar stains were found on the hands of her husband, fixing the crime of murder upon him. In another case, a man was accused by a woman of attempting to poison her by putting copper into her soup. Chevalier examined the soup and detected an abundance of the poison in it; but not a trace of the copper could be discovered on the surface of the pot in which the soup had been boiled, or in the soup still remaining in it. It was consequently clear that the woman, who had some spite against the man, had herself introduced the poison.

(TO BE CONTINUED.)

SELECTIONS

FROM

AMERICAN AND FOREIGN JOURNALS.

Rupture of the Uterus and Abdominal Parietes.—Cæsarean Operation performed with Success, both for the Mother and Infant. Thirteen months afterwards, Rupture of Uterus and Abdominal Parietes, from a second Pregnancy; Birth of the Infant by this spontaneous Opening, and perfect Recovery of the Mother. By Dr. Pearl, Director of the Obstetrical Institution of the city of Hildesheim.

A news vender, ætat, 28, enjoying good health, both during her early infancy and adult life, but deformed from rickets, in whom the catamenia appeared at the age of 13, entered the midwifery Hospital of Hildesheim, far advanced in pregnancy, shortly expecting to be brought to bed. A careful examination satisfactorily proved that the great diameter of the true pelvis, "du petit basin," was only two and a-half inches. It was, therefore, probable that she could only be delivered by the Cæsarean operation. Labour set in by powerful but useless pains, and the fœtus furnishing abundant proofs of life, the operation was determined on. Accordingly an incision was made in the direction of, and upon the linea alba; the peritoneum and the uterus were opened upon a director (sonde connelee,) and without the slightest lesion; a living female child, well formed, and arrived at full time, was drawn forth. By a slight traction upon the umbilical cord, the placenta was extracted through the same opening. The hemorrhage did not amount, at most, to a pound of blood. The edges of the uterine wound were then brought together as closely as possible by means of six "points" of suture, care-

fully avoiding the peritoneum. The lips of the tegumentary wound were placed in apposition, leaving, however, a small opening at the inferior angle, for the escape of any fluid that might otherwise accumulate within. During the operation, which was borne with the most heroic courage, the patient experienced only one slight nausea; she was not so much exhausted by it, and declared that she felt herself quite well. On the day following, the infant was applied to the breast, which it took with evident satisfaction. The issue of this accouchment was of the happiest nature, the mother suffering but a slight accession of fever, caused by an accumulation of milk; her infant refusing the breast, from being attacked with trismus nascentium, of which it died on the ninth day. The mother recovered perfectly, but slowly; at the expiration of two months from the operation there was complete cicatrization of the wound; at that of eight months, a reappearance of the catamenia; and in the ninth month, she left the hospital in excellent health, at which time the cicatrix was five inches in length and from half to three-quarters of an inch in width. In the beginning of the following year, she again became pregnant. During the first four months which followed, she enjoyed such good health, that she, for the first time noticed her state, by the movements of the infant in the twenty-first week. These movements were accompanied by violent dorsal pains, lasting but for a few minutes only; she paid but little attention to them, from having experienced similar at the age of sixteen. She now noticed an ulcerated spot upon the skin, which was slightly swollen; it was situated at the right side of the abdomen, at the distance of the width of the hand from the Cæsarean cicatrix. This ulceration daily extended in points, pouring out pus, and sometimes blood, causing but little or no pain; simple dressing was applied.

On the 14th of July, 1843, having taken cold, she had an access of fever, accompanied with pains in the sides, back, and abdomen, for which Dr. Shrœder was called in; he found that part of the abdominal integuments, which we have described, œdematous, and of a reddish-brown colour, and about the width of the hand in extent. The fœtus could be so distinctly felt through the abdominal parietes, that on first consideration it was considered to be extra-uterine. A solution of muriate of ammonia in liquorice juice was ordered, and the wound dressed with a mild ointment. The same evening, pains having the character of labour-pains, set in, but disappeared on the 16th. The muriate of ammonia was continued, causing a sensible diminution of the cough, and febrile excite-

ment. In a short time, the patient was able to leave her bed; the movements of the fœtus were no more felt.

On the 18th, she experienced a pressing desire to make water, and go to stool; she arose, made a few steps forward, and called for her sister to hand her the vessel, when suddenly a crack (*craquement*) was heard. accompanied with a rupture of the abdominal parietes, through which the fœtus, enveloped in its membranes, could be felt. In this fearful dilemma, alone, and left to her own resources, with both hands, she retained the infant in the rupture. until help arrived. A midwife was called, who found the patient standing, the membranes being torn, she had merely time to take away the infant, which was dead. Dr. Shrœder, who arrived one hour after, found a large rent in the abdomen, situated below the umbilicus, of a transverse direction; the membranes, and a part of the omentum, protruded at the right, whilst the cord, and the placenta, still adhering to the uterus, appeared at the left. Fearing lest upon the extraction of the placenta, the intestines might escape, he called in Dr. Prael, on whose arrival, the patient, notwithstanding all she had suffered, was yet in a state not to be despaired of. She was pale, and the pulse was small; but there was not any thing in her manner, or the expression of her countenance, which indicated particular danger; the hemorrhage had not been very abundant, the wound smarted a little, but was not painful. The solution of continuity was at least five inches long, extending one and a half inches to the left side of the cicatrix, and three and a half inches to the right; its lips were so separated, that at first view, it was not easy to say whether it was transverse or perpendicular; its edges, particularly the inferior, were œdematous, swollen, turgid, turned inwards, and unequal; below this opening, and about an inch from its edge at right, there were two patches of extravasted blood, both transverse in direction, and one inch and a half in length. At the right of the centre of the rupture, a tumour, of a violet colour, and of the size of the head of a new born infant, was seen; it was the membranes, filled with coagulated blood: behind and above this, a portion of the omentum, as long as the hand, and the width of three fingers, protruded, the placenta in part visible, and a considerable portion of the cord filled the left side of the wound; there was no escape of the intestines. As the cord was so thin, that it could not bear the slightest traction, without danger of being torn, it was necessary to introduce three fingers into the uterus, through the abdominal opening, in order to remove the placenta. The uterus had

already so much contracted, that it was quite impossible to learn, with certainty, the direction which the rupture had taken in it; the placenta, which was attached to the posterior wall of the uterus, gave trouble in its removal, and caused considerable pain to the patient.

During this operation, no part of the intestines protruded. In order to shun any such accident, the operator made the examination as short as possible, confining himself to washing away the blood, bringing the edges of the uterine opening together, and returning the omentum in the best way he could. The edges of the abdominal wound were so puckered (*plissès*), swollen, unequal, and œdematous, that sutures could not be used, they were therefore held together, by adhesive straps, placed perpendicularly. Having cut away with a scissors a small portion of the peritoneum, which was only held by a few filaments, at the right angle of the wound, the dressing was completed by soft compresses, and a bandage. An examination of the genital organs was next proceeded with, but nothing was found in the vagina. The orifice of the uterus was smooth; the os was dilated to about the width of two fingers, through which there was no flow of blood, or any other fluid. The reaction which followed so serious a lesion, was held within due limits, and there was but little fever. The state of the wound, however, gave rise to the most serious apprehension. At the removal of each dressing, a considerable quantity of sanious sanguineous fluid was discharged; its edges assumed an unhealthy aspect, and the patient complained of severe pains in the loins.

The opening, becoming gangrenous, gave issue to a very fœtid sanious discharge; however, with the exception of the stools, all the functions were normally performed, and the lochiat ransmitted through the natural passage. Quinine was administered, under the exhibition of which the aspect of the wound gradually changed, assuming a healthier appearance, and pus of a good quality was secreted. The quinine was now obliged to be discontinued from an attack of rheumatism of the left side, but was afterwards administered with good effect, notwithstanding an abscess formed in the left inguinal region, carrying in its train a swelling, and paralysis of the leg of that side. Emollient poultices were applied, but the œdematous swelling progressed, extending over the entire abdominal integuments. The process of cicatrization, which had reduced the wound to the size of a two-franc piece, was also manifestly arrested. By the aid of tonics, antiseptics, and a nutritious regimen, the œdema disappeared in the

course of eight days; sleep and appetite now returned, her strength augmented, cicatrization recommenced, the quantity of pus daily diminished, and there only remained a few cutaneous ulcerations in the neighborhood of the rupture, which were soon healed by the application of the nit. argenti. In one month after she was so far re-established in health as to be able to quit her bed; and in fifteen days more, the wound was reduced to the diameter of a one-franc piece.

She was now enabled to be engaged in domestic affairs, and walk for half an hour each day. On the 5th of October, cicatrization was complete, she felt no pain, and was fast regaining her *embonpoint.* The catamenia appeared in six weeks after the accident, and again towards the end of October without pain, but less abundant than the first time. She now left the hospital.

This extraordinary case is so much the more interesting, as it is perhaps the only one in the annals of medical science. One exhibiting some analogy is related in an old English work, little known, published by the Society of Edinburgh.

The translator, not without some difficulty, from the indefinite description given of this work, found the paper referred to in vol. ii. of the "New Edinburgh Essays," p. 338, published in 1756. It was read before the Society by Alexander Monro, senior, and is thus given:

"In April, 1736, Elspet Grant, in the parish of Moy, being with child, took her labour pains. After they had continued three days with the child in birth, two cracks, as if the rafters of the house had broken, were heard about the sick wife, and her belly was rent from near the navel, with a squaint downwards, and to the left side to near the share bone. At this rent the child came into the world, the after-burden was brought away, and the entrails were seen. The woman recovered."—*Dub. Jour.*

From Mr. Paget's learned Report on the Progress of Human Anatomy and Physiology in the years 1843–4, in the British and Foreign Medical Review, we make the following extracts:

Color of the Blood.—Some experiments by Scherer both confirm the opinion of Nasse, that the change from the arterial to the venous colour of the blood depends in a great

measure on the form of the blood-corpuscles, and explain most of the observations of Dr. Stevens on the effects of distilled water and salts upon the blood. Their general conclusions are; 1. That when fresh-stirred and bright-red ox-blood is mixed with distilled water, it acquires a dark-red colour, and its corpuscles, by imbibing water, become spherical, and at last vanish. But, 2, That if, after the change has begun, and not gone far, a concentrated solution of a neutral salt be added, the blood-corpuscles again acquire their natural form, and the bright-red colour is restored. 3. That when oxygen is passed through blood darkened by the addition of distilled water, it is not changed in colour, and the blood corpuscles do not reappear; but that the same kind of blood, mixed with a small quantity of milk, or oil, or finely-powdered chalk, or gypsum, soon regains its bright-red colour. 4. Again, by the long-continued contact of concentrated saline solutions with blood-corpuscles they become jagged and decomposed, and the blood becomes black; and those which have been reddened by the action of salts, become black again on being expanded by the imbibition of water. 5. By adding carbonic acid to bright-red blood, its corpuscles change their biconcave for a biconvex form, and at the same time its colour changes from red to black. So that there are always changes in the shape of the blood corpuscles, coincident with the changes in the colour of the mass of blood; whenever they are dilated, as by distilled water or carbonic acid, the dark colour is produced; whenever they are contracted into the biconcave form, the bright-red colour is restored.

Mulder, also, espouses the opinion of the changes of colour in the blood being immediately due to physical rather than to chemical changes of the corpuscles, and has added many facts to those just quoted in disproof of the opinion of Liebig, that the changes are due to the alternate production or the carbonate of the protoxyde, and of the peroxyde, of iron in the blood-corpuscles, as they pass alternately through the systemic and the pulmonary capillaries. His chief facts are—1. That the elementary composition of the colouring matter is the same, whether obtained from arterial or venous blood, viz., C. 44, H. 44, N. 6, O. 6, Fe. 2. That the change from dark to bright blood is effected as completely by the agency of a neutral salt as by oxygen. 3. That if the iron were present in the blood as an oxyde (and especially as a peroxyde), it should be easily extracted by weak acids; but he has found that well prepared hæmatine may be digested in diluted hydrochloric or sulphuric acid for several days without the

iron in it being in the least diminished. After being so treated he has obtained, after incineration, the regular proportion of 9-49 per-cent. of oxyde. If strong sulphuric acid be poured on dried blood, or dried pure hæmatine, and kept on it for some days, and then water be added, hydrogen is evolved, and sulphate of peroxyde of iron is found in the solution, which could not happen if the iron had been at first in the form of peroxyde. 5. The iron may thus be all extracted from the blood, or from hæmatine, (though not as some say, without affecting the colour,) and the other constituents may be obtained in a separate form. Numerous analyses of this constituent, by Van Goudover, regularly yielded the same equivalents of the elements, viz., C. 44, H. 44, N. 6, O. 6; but if the iron had been united with this in the form of Fe. 2, O. 3, and in the proportion of one equivalent to two, there should have remained only four and a half equivalents of oxygen.

Mulder concludes, therefore, that iron is present in hæmatine, as iodine is in sponge, or sulphur in cystine, or arsenic in cacodyl. His notion of the mode in which the changes of colour are effected is, that when the corpuscles of the venous blood are exposed in the lungs, oxy-proteine is formed by the oxydation of the fibrine-proteine of the liquor sanguinis, or, perhaps, by the oxydation of the outer layer of the cell membrane of the corpuscles. If formed on the liquor sanguinis, its peculiar plasticity would lead to its being deposited in a thin layer on the corpuscles. In either such case, the dark corpuscles would, after respiration, be invested by a thin layer of white and imperfectly transparent oxy-proteine, or buffy coat, through which they would look bright red, as dark blood does when contained in a vessel of milk-white glass. But, in the systemic capillaries, the oxy-proteine may be consumed in nutrition, and the darkness of the corpuscles will then again appear unveiled.

Moreover, since it appears that, in the biconcave form, the corpuscles by reflecting more light, are always bright, and in the biconvex form always dark, it may be that in the arteral blood they are not only *buffed* but also *cupped*, by the oxy-proteine, [by the plastic properties of which, morover, it is easy, on this pretty theory, to explain the ready adhesion of the corpuscles in inflammatory blood.] Diluted acids, which make bright blood dark, may do so by making the outer layer of the corpuscles transparent, as they do fibrine before dissolving it; and concentrated solutions of neutral salts may make it bright by making the same layer contract.

Animal Heat.—An extensive series of observations has been made by M. Roger on the temperature, of children in health and various diseases.

In nine examinations of infants from one to twenty minutes after birth, the temperature, (observed in these and in all other cases, in the axilla,) was from 99-95 to 95-45. Immediately after birth the temperature was at the highest; but it quickly fell to near the lowest of those above stated; but, by the next day, it was again completely or nearly what was before. The rapidity of the pulse and of respiration appeared to have no certain relation to the temperature.

In thirty-three infants of from one to seven days old, the most frequent temperature was 98·6; the average was 98-75; the maximum (in one case only;) 102-2; the minimum, (also observed only once 96°-8. All the infants were healthy. The frequency of resperation had no evident or constant relation to the temperature. A few of the infants were of weakly habit; their average was 97-7, the others were strong, and their average temperature was 99°-534. The age of the infant (in this short period) had no influence on its temperature; neither had its sex, nor its state of sleep or waking, nor the period after sucking.

In twenty-four children, chiefly boys, four months to fourteen years old, the most frequent temperture was above 98°-6; the average was 98°-978; the minimum was 98°-15; the maximum 99°-95. The average temperature of those six years old or under, was 98°-79; of those above six years old, 99°-158. The average number of pulsations in the minute was in those under 6 ears old 102; in those above that age 77; yet the temperature of the latter was higher than that of the former or of younger infants. There was no evident relation between the temperature and the frequency of respiration; nor, in a few examinations, was the temperature affected in a regular way, by active exercise for a short time, or by the stage of digestion.

As already said, in all the examinations from which these results were obtained, the thermometer was held in the axilla; comparative examinations proved that the temperature of the axilla, (though lower than that of internal organs,) was higher than that of any other part of the skin. Of the other parts examined, the warmest was the abdomen, then in succession, the cavity of the mouth, the bend of the arm, the hands, the feet; of which last, the average temperature, in four examinations was only 87°-35. [These results correspond sufficiently with those obtained by Dr. John Davy.]

In diseased states, (to the illustration of which the greater

part of the memoir is devoted,) the temperature of the skin in children may descend to 74°-3, and may ascend to 108°-5. Its range of variation is therefore much greater than in adults, in whom M. Andral found it to vary in different diseases not more than from 95° to 107°6.

Dr. John Davy also has contributed some miscellaneous observations on animal heat. They are made with a thermometer placed beneath the tongue. In eight old men and women, all, with one exception, between eighty-seven and ninety-five years of age, the temperature was 95° or 98°-5; therefore not below the average of persons in like circumstances. But two observations showed, that in exposure to external cold, the temperature was more reduced than in young persons, in one case to 95°, in the other to 96°-5. A few observations were also made on persons working in rooms at a temperature of 92°; in one case the temperature under the tongue was 100°, in another 100°-5; and in a third, with an external temperature of 73°, it was 99°. The same slight variations of the temperature of superficial parts in accordance with changes of external temperature were shown by repeated observations on a healthy man in the different seasons at Constantinople. By moderate exercise the temperature was raised, (but not above the general average,) on the surface of the extremities, and was not affected in the internal parts.

Some interesting observations on the effects of hot moist air upon the body were made by M. Constantin James, at the baths or stoves of Nero, called formerly Posidianæ, near Pozzuoli. The physiological part of his account is, that he had first to traverse a passage, leading to the hot springs, about seven feet high, and three wide, in which the temperature in the upper strata of air was 104°F. in the lower 91°-4. After going along this for about fifty yards, the passage narrowing and winding, the temperature in the different strata became 109°-4, and 98°-6, and was very inconvenient; his pulse increased from 70° to 90 times in the minute. After a few instants, as he went on, his pulse became 120°, his temporal arteries beat forcibly, his respiration was short and panting, his body bedewed with sweat; he was obliged to stop every instant, and put his head to the ground, where he could breathe the least heated air. The temperatnre had now become 118°-4 in the upper, and 111°-4 in the lower strata, and the atmosphere was filled with dense vapour. Still descending towards the spring, the atmosphere became still more suffocating; his head felt bursting, he was utterly exhausted, and had nearly lost his consciousness; his pulse

could not be counted. The temperature of the spring was 185°, while that of the atmosphere was 122°. Summoning all his strength, the experimenter made his way back through the passage, in which he had been for nearly a quarter of an hour, and of which the whole length was about 120 yards. On coming into the cold air he nearly fainted, and staggered like one drunk, till he was relieved by bleeding at the nose; but through the evening his pulse was 100, he was feverish, had ringings of the ears, and a kind of creeping in the limbs. After a good night's rest he was completely recovered. The water from the spring was clear, and not charged with any gas; neither was any deleterious gas mingled with the heated air.

M. James compares these observations with those made on rabbits and other animals, by M. Magendie. The chief results of these experiments were as follows: two rabits, of the normal temperature of about 102°-2 were placed in two stoves, one at 212°, the other at 140°; the first died sooner than the second, but the temperature of each at the instant of death was the same, namely 111°-2. And the same experiment often repeated, showed that whatever were the ratio at which the heat was applied, the animal died when this increase of nine degrees was attained. In birds whose normal temperature [in some,] is 111°-2, i. e. the same as that at which mammals die, death ensues upon the same increase of nine degrees of temperature; they die when their blood is at 120°-2.

If when an animal is near dying from the effect of heat, an artery be opened, its blood is as black as that of a vein, it does not congulate, and does not become bright on exposure. Exposed to dry heat, the animal loses much weight by evaporation before it dies; exposed to the moist heat it loses no weight, but dies sooner. In the vapour baths of Nero, M. James was almost suffocated in a temperature of 112°, while in those of Testaccio, in which the air is dry, he was but little discomforted by a temperature of 170°; and this has been confirmed on many animals. The quantity of weight lost by evaporation in hot dry air is directly proportionate, not to the temperature, but to the duration of the exposure, and the rate of loss is the same during all the times. After death, the lungs and heart are found in the contrary state to that seen after death from cold; they are empty from blood, and it is collected and extravasated at the surface of the body.

Analysis and properties of the gastric fluid.—The most remarkable results of M. Blondlot's investigations relate to the composition of the gastric fluid, and different as his conclusions may be from those usually received, yet the large quantity of fluid he was enabled to collect in a purer state than any one hitherto has collected it, entitles his account to every consideration. He very carefully distilled on a sand-bath 3875 grains of pure gastric fluid obtained after feeding his dog with raw meat; he repeated the distillation, and repeated the whole experiment, several times, with the gastric fluid of other animals as well as of the same dog, and the constant result was, that the product of the distillation did not once exhibit the slightest acid reaction; but the residue in the retort was always strongly acid. It was thus proved that the acid of the gastric fluid cannot be either the hydrochloric or the acetic, for both these are volatile at the boiling point of water, and would have distilled over.

The general conclusion of his analysis is, that the gastric fluid is composed of ninety-nine parts of water, with one part of super-phosphate of lime, super-phosphate of ammonia, chloride of sodium, mucus, an aromatic, and a peculiar, principle.

Similar results were obtained from the analysis of the gastric fluid of several animals.

Besides these experiments concerning the chemical properties of the gastric fluid, M. Blondlot relates others, which add to the evidence already known, that the real digestive property of the fluid depends, not on its obvious chemical qualities, but on an organic principle. If exposed to a temperature between 104° and 122° F. or higher, it entirely and irrecoverably loses its digestive powers, although apparently, and as to analysis, unchanged. Kept from the air, the gastric fluid retains its active properties for at least two years; but, exposed to the air and a moderate temperature, it putrefies in five or six days, although the chyme which it forms from nitrogenous organic substances may be preserved for two or three months without apparent change. The precipitation of all the lime which it contains does not affect its activity; neither are its chlorides indispensable; but whatever much alters its organic constituents, (such as heat, strong alcohol, or strong acids.) or removes them, (such as animal charcoal, tannic acid, chlorine, or acetate of lead,) destroys all the digestive properties.

Digestive properties of the gastric fluid.—Some singular evidence of these is furnished by MM. Bernard and Barreswill

who have found that nutritive substances injected in simple aqueous solution into the blood are not assimulated: but are assimulated if dissolved by the aid of the gastric fluid. Among other experiments are these: portions of cane sugar, albumen, and gelatine, seven and a half grains of each, and severally dissolved in water, were injected into the jugular veins of three dogs. Three hours after, the urine of each was examined, and in each the injected substance was found. Under other similar conditions, the same quantities of the same substances dissolved in gastric fluid were injected, and three hours after, gelatine was detected in the urine of the dog into whom it had been injected, but not a trace of albumen or sugar in the urine of either of the others. Three dogs were then fed exclusively and respectively on gelatine, albumen, and sugar; and the first alone could ever be detected in the urine. The authors fed themselves in the same way and obtained the same result; and they conclude (as others presently to be mentioned do) that gelatine is not assimilable and therefore not nutritive.

Among the experiments which M. Blondlot made to determine the mode in which the gastric fluid, in or out of the stomach, acts on different animal substances some afforded novel and interesting results; A. He shows that coagulated albumen owes its long resistance to the digestive fluid only to its compact form. When coagulated in very fine particles (as by pouring white of egg beaten into a froth, into boiling water) it is digested as quickly as soft fibrine. B. He adds further evidence that the action of the stomach in coagulating milk is not due peculiarly to its digestive principle, but to its acid, which acts like the lactic acid developed from the sugar under the influence of rennet or any other decomposing azotized compound: C. The effect of the gastric fluid on bones, observed both on the bones in their entire state, and on their animal and inorganic constituents separately, is, that first, it very slowly disintegrates the animal matter, attacking them from the surface, and then, also very slowly, disintegrates and reduces the earthy matter into a fine chalky powder, but without either dissolving or decomposing it. The earthy matter not being dissolved proves that no hydrchloric acid had acted upon it: and in its minutely divided state it all passes through the intestines and is discharged with the fæces.

The result of many more of his experiments of this kind are interesting. They confirm Mr. Beaumont's, and appeared to M. Blondlot to show that of all the simple alimentary sub-

stances, those which are fluid at the ordinary temperature of the stomach, and those which are easily soluble (in the ordinary manner of solution) in its secretion, such as fluid albumen, sugar, gum, pectine, &c., are at once absorbed by the veins; and that others, which are not liquid nor easily soluble, such as fibrine, coagulated vegetable and animal albumen, caseine, gelatine, &c., are, in only a very small proportion, if at all, dissolved, the action of the gastric fluid on them being limited to the softening of them, so that they are reduced into very minute particles which (out of the stomach) appear like a very fine precipitate. The same general rule is said to be observed in the digestion of the compound alimentary substances, both animal and vegetable; the fluid and easily soluble parts cannot be said to be digested, for they are at once absorbed by the stomach; the rest are softened and reduced into very minute particles, which are carried into the intestines, without any change in their chemical constitution, and are, in this state absorbed by open mouths of the lacteals, visible with the naked eye, at the extremities of the villi(!) This act of softening is, in some cases, due merely to the acid of the gastric fluid: e. g. in the case of parenchymatous tissues, and succulent fruits and roots, which are similarly softened, at the same temperature, in acidulated water; in the cases of fibrine, coagulated albumen, &c., it is the effect of the *peculiar* mode of action of the gastric fluid.

In any case, chymification is, in M. Blondlot's opinion, no *solution*, but a *division* of the aliment; it undergoes no kind of decomposition.

The physical position of man in creation.—The law of unity of type and function in animals, applied in the preceding pages to the function of the cerebro-spinal axis in man, has shown (what is necessarily deduced from the law itself) that the transition of structure and function is gradual, and consequently, no strong line of demarcation can be drawn between the manifestations of its various functions. The automatic acts pass insensibly into the reflex, the reflex into the instinctive, the instinctive are *quasi* emotional, the emotional are intellectual. This gradation of structure and function observed in the nervous system, is observed also with reference to all other structures of his body. Man is at the head of a vast ascending scale of animal life, so extended in its connexions downwards, that for the present purpose it may be considered as infinitely extended. With our existing knowledge of

of the uniformity of the laws of creation, the deduction is absolutely incontrovertible, the scale of being is not truncated at man, and that beyond him there cannot be a dark, unpeopled void. The law of gradation of development vigorously pushed to its legitimate conclusions points out an infinite gradation of being *above* and *superior* to man. That we cannot see such beings, nor demonstrate their existence is a necessary result of our position in the scale, and no proof whatever of their nonexistence. The worm knows nothing of man, his works or his actions: nothing of the sun or the stars. of the beings swarming around it; and so with reference to the spiritual world, the world around and above *us*—our organs may be and doubtless are, as imperfect as those of the worm with reference to the world around and above *it*. Man is then at the foot of another scale of beings, the highest of which *at least*, as far transcends man, as man transcends the zoophyte. This proposition, I repeat, is the unavoidable inference from our present physiological knowledge, and is a complete answer to those good, zealous, but not wise men, who think science leads to scepticism and irreligion. It leads to a rational faith utterly opposed to arrogant infidelity.—*Dr Laycock in Brit. & For. Med. Review.*

Raciborski on Puberty.—He next notices (p. 77) cases of extraordinarily precocious menstruation of which he has collected many cases from various authors, and finds in the theory he has adopted with reference to the cause of menstruation, a ready explanation of these exceptional occurrences. Menstruation which is the index of aptitude for generation appears last in the train of changes, and when all the organs have acquired their due development. In some countries these changes take place earlier than in others, and there seems to be no reason for regarding menstruation during infancy as any thing else than a highly exaggerated degree of precocious puberty.

In both cases the premature appearance of the menses seems to depend on the precocious development of the ova and of the Graafian follicles, which give, as we know, the impulse to all the other organs of the reproductive apparatus: only in one case this precocity evidently depends on the influence of climate or temperature, while in the other it appears to be the effect of congenital peculiarities. One circumstance in favor of this opinion is the invariable coinci-

dence of development of the mammæ and external genital organs with the hemorrhage, in cases of premature menstruation. These phenomena, indeed, do not occur until the follicles of Graaf are already on the point of attaining that full degree of development which characterizes the period of puberty; they are absent, or exist only in a rudimentary degree, whenever the development of these follicles is impeded by some primitive disposition of the economy; by diseases, or by the congenital absence of the ovaries, or their removal.

In support of this opinion too, he observes that though the Graafian follicles are usually not formed until after birth, he yet found a perfectly developed Graafian vesicle on one occasion in the ovary of a child still-born at the seventh month of pregnancy.

Now every thing is in favor of the supposition that had this child continued to live, it would have presented several follicles at the conclusion of pregnancy, and it is very probable that if the same degree of vital energy had continued, it would soon have arrived at the end of some months of extra-uterine life; at that state which is not usually attained before the age of fourteen years. That which we observed as an exception to the general rule in the above-mentioned fact, may occur in all cases of premature menstruation. It may be said, indeed, that in all cases of this kind, the most important anomaly consists in the premature development and precocious maturity of the ova; the menstrual hemorrhage is only the necessary consequence of this anatomical and physiological peculiarity."

It must not however be inferred that the converse of this holds good, and that in women in whom the occurrence of menstruation does not take place till a very late period, there is a corresponding delay in the maturity of the ova and Graafian follicles.

"So far indeed, from that being the case, these facts seem only to confirm what we shall hereafter prove more directly, namely, that the menstrual hemorrhage is only a secondary phenomenon attendant on menstruation strictly so called, and that the capital phenomenon of this function consists in the periodical maturity and elimination of ova. There are some women in whom nothing else occurs besides this phenomenon and authentic cases are on record of women who have given birth to several children without having ever menstruated. Still stronger reasons are there for admitting the possibility of those cases in which, after the ovaries have for a length of time performed their monthly function unattended by any other

phenomenon, menstrual hemorrhage at length occurs. Many other signs of puberty no less valuable than the flow of the menses serve to indicate the existence of that state when the menstrual flux is absent.

Long continued illness, it is well known, often has the effect of retarding the epoch of puberty, even after some of its precursory symptoms have appeared, or of suppressing the menstrual flux, although it may have already occurred once or twice. In such cases, the development of the Graafian follicles is found to have been arrested, as was observed by M. Raciborski in the case of two chlorotic girls who died between the ages of sixteen and seventeen; and in whom the Graafian vesicles, though as numerous as they should be at the period of puberty, were of extremely small size. In women in good health, however, menstruation when once established continues for the most part to recur regularly every month. Having noted the interval between the first two menstrual periods in eighty-seven women, M. Raciborski found that, in fifty eight, this interval did not exceed one month. In two women the second menstruation followed six weeks after the first; in four, after an interval of two months; in five, after three months, in four, after four months; in one, at the end of five months; in another, at the end of eight months; in three, at the end of one year; and in another, two years had elapsed. From these data it appears that, from the first appearance of menstruation, it recurs regularly every month in about two thirds of all healthy females.

Festival to Commemorate the introduction of the Potato.—Several of the German States have, we learn from the *Atheneum*, instituted feasts in honor of the introduction of the potato; and the anniversary of its importation has just been held as a jubilee at Bavaria. At Menterschwaige, near Munich, a festival was observed on the occasion in which dishes of the poor man's especial root, variously dressed, had the place of honor on the table:—while the bust of Sir Francis Drake, crowned with garlands of oak, and presented to the commune, for the occasion, by its sculptor Schwanthaler, occupied the centre of the hall. In France, a monument is about to be erected to Parmentier, commemorating its introduction into that country. It may appear to our readers, that the honor paid to the memory of Drake was really due to Raleigh; but it is probable that the Germans are literally correct. The

first colonists, sent out by Raleigh, were disheartened when Drake touched at Virginia, and he consented to bring them home. Lane, the governor, who is believed to have brought with him the first tobacco, may, and probably did, bring the first potato; if so, though indebted to the enterprise of Raleigh for the discovery, it was Drake's ship that actually introduced the first root.

Proportion of the Insane.—Dr. Thurnam read a paper on the Statistics of Insanity, designed to refute Esquirol's theory that women were more predisposed to insanity than men in the proportion of 38 to 37. The discussion of this point turned chiefly on the corrections necessary to be made in statistical tables of insanity by taking into account the rates of mortality, the proportions of male and female population, and the probability of recovery in the two sexes. Dr. Thurman came to the conclusion that Esquirol's proportions ought to be reversed; but the question ultimately resolves itself into whether more confidence is due to the statistical returns of insanity in France or England..

In the discussion that ensued, Mr. Tuke entered at length into an examination of the alleged disproportion of insane among the Society of Friends, tending to prove that the predisposition to mental alienation in that society is less than in the general community.

In the absence of Col. Sykes, Mr. Heywood read his paper on the Lunatic Asylums of Bengal. They are chiefly remarkable for the rapidity with which they have improved since their first establishment, and the very trifling cost at which they are supported.—*Meeting of the British Association for the Advancement of Science.*

Dr. Home.—Dr. James Home, late professor of Medicine in the Northern University, was at his death, on the 5th of December, eighty four years of age. It is only two years since he relinquished his chair. He was a professor for forty years, having succeeded his father, Francis, in 1779. On *Materia Medica*, Dr. Home was very popular in the University, particularly during the last years of his holding that professorship. This increase of popularity was chiefly owing to his having adopted the new views of Sir Humphrey Davy on the subject of chlorine. For while Murray, of the Edinburgh School, keenly opposed those views, seeking to overthrow

them by experiment, and Hope hesitated to adopt them, Home remodelled this part of his lectures, and so closely followed Davy's nomenclature and explanations, that the student did not learn from him that there ever had been any other. In the chair of Medicine, no teacher took greater pains with his lectures; yet their success latterly was far from proportioned to the pains bestowed upon them. In the clinical wards, he had the reputation of being attached perhaps a little too much to the lancet.—*Northern Jour. of Med.*

Hooping-Cough, its Treatment and Effects.—Mr. Roberts, in reference to the discussion of the last meeting of the Medical Society of London, explained that he considered dilated bronchial tubes were never found in young persons, but were not uncommon in those who are the subject of chronic bronchitis, or the old catarrh or winter cough, as it was called. Assuming this to be correct, it was reasonable to suppose that pectoriloquy occurring in young persons was the result of a cavity.

Dr. G. Bird differed in opinion with the last speaker, for he had found that the bronchial tubes were not uncommonly dilated, even in children, and particularly in those who had suffered from protracted hooping-cough. He had found this dilatation at all ages, from the period of dentition to puberty.

Dr. Wiltshire agreed in the main with Dr. Bird, and expressed his belief that the mass of cases of mild consumption, in which the physical and other signs and symptoms of phthisis were present, were, in fact, cases of dilated bronchial tubes. He had seen the most severe forms of chronic bronchitis between the ages of twenty-five and thirty.

Mr. Crisp had examined several children who had died of hooping-cough under the age of three years, and had never found dilated bronchial tubes.

Belladonna in Hooping-Cough.—Dr. Waller had of late tried belladonna in two cases of hooping-cough with the best results. He gave the extract in a twelfth of a grain dose, three times a day, to a child four years of age. In these cases there was simply the spasmodic cough, and no indication whatever of the presence of inflammation. Prussic acid and conium had failed in affording any permanent or marked relief.

Mr. Crisp had in the majority of cases found the antiphlogis-

tic plan of treatment in the early stages of hooping-cough to be the best treatment. In robust and healthy children the application of leeches, followed by blisters, and the use of tartar emetic, had usually been attended by the best results. He viewed the disease generally as inflammatory, or at all events congestive. Contrasting the antiphlogistic treatment with that which he had formerly pursued, he had every reason to be satisfied with the change that he had made. With respect to the use of prussic acid, he had usually found it of service only for a day or two.

Dr. Wiltshire, at the Infirmary for Children, had treated simple uncomplicated hooping-cough with two or three emetics of ipecacuanha, one every morning or second morning, followed for two or three days with nauseating doses of antimony, and had found this plan of great service in the early stages of hooping-cough. Hemlock and ipecacuanha were of benefit afterwards in relieving the cough. With respect to belladonna, he was fearful of its employment, for notwithstanding the evidence in its favor, adduced by certain German practitioners, there were others who had found that this medicine has a tendency to increase vascular action in the brain, and to the production of hydrocephalus.

Dr. Chowne, in the treatment of hooping-cough, laid particular stress on the necessity of keeping the patient in a warm temperature, and using every means to prevent his catching cold. Nauseating doses of ipecacuanha were frequently of benefit. With reference to the production of emphysema by hooping-cough he did not believe it possible.

Dr. Clutterbuck looked upon hooping-cough as a specific disease, produced by a specific cause—it was an inflammation of the bronchial membrane of a specific character, but apt to induce inflammation of other organs, such as of the lungs or of the head, and it was these complications that constituted the dangerous characters of hooping-cough. He had little confidence in any remedy for this affection. The disease should be narrowly watched, with the view of prevention, rather than of active treatment; if mild, nothing was necessary to be done, it would terminate spontaneously; if threatened the complications he had alluded to decided antipholgistic means were demanded.

Dr. Golding Bird regarded the hoping-cough, in the first stage, as invariably inflammation of the lining membrane of the bronchial tubes, larynx, and trachea, of a specific character, and implicating, in some manner, the par vagum. This inflammation lasted a definite period, which was influenced by

constitution, and other causes. In the second stage of the affection, the disease was *nervous*, the specific irritation of the par vagum being kept up, altogether independent of inflammation; or, if this were present, it was accidental; the hooping was afterwards protracted, by the influence of habit. In the first stage, remedies for bronchitis were advisable—such as emetics, diaphoretics, the warm bath, and a warm temperature. When the inflammotory stage was passed, the object of the practitioner was to subdue irritation in the par vagum, and this was effected by the agency of narcotics—such as conium, in conjunction with the carbonate of potash, hemlock, and hydrocyanic acid. Embrocations to the spine and chest were also useful. When bronchorrhœa became troublesome, small doses of alum, with sedatives, were employed with advantage. When the bronchorrhæa had ceased, tonics were indicated,—the kind of tonic to be determined by the constitution of the patient. Emphysema did not occur as the consequence of hooping-cough.

The President had found, in some cases where belladonna was given, that the poisonous, rather than the curative effects of that remedy developed themselves, even though the doses administered were remarkably small. With reference to the irritation of the par vagum in hooping-cough, he related a case in which this nerve had become exposed, from the formation of an abscess, or other cause; and it was remarkable, that when the nerve was in contact with the air, a spasmodic resembling hooping-cough was produced: when the nerve was covered over by a cicatrix, the hooping-cough ceased.

Prognosis of Chancre with reference to the probabilities of Secondary Symptoms.—By M. Ricord of Paris—In the report of M. Ricord's lectures, it is stated as the result of his extensive experience in the Venereal Hospital, that he has arrived at the following conclusions relative to the chances of secondary symptoms after a primary sore.

1. Primary ulcer is the indispensable precedent of secondary syphilis; without chancre there can be no general infection, except in the rare case of hereditary disease.

2. Simple non-indurated chancre and gangrenous chancre are very seldom, and only in exceptional cases, followed by secondary syphilis.

3. Indurated chancre always gives rise to constitutional infection.

4. The seat of chancre does not in the least degree influence the production of secondary symptoms. Provided the chancre be indurated, secondary disorder is as common and as constant after sores of the mouth, hand, nose, or foot, as after chancres of the penis.

5. The size of a chancre, the number of sores a person is affected with, do not increase his chances of general infection, always provided none be indurated.

6. If the primary sore be destroyed during the six first days of its existence, no secondary symptoms will follow.

7. If six months elapse after the cure of a chancre (no mercury having been exhibited,) without the appearance of secondary eruptions, all fear of constitutional symptoms may be laid aside.—*Ibid, from the Prov. Med. Jour.*

The Engrafting of Nerves.—M. Flourens, in reference to some experiments made by M. Tavignot, proving the possibility of engrafting nerves one on the other, reminded the Academy that he had published, some years since, similar experiments, with like results. He had seen the interlaced reunion of several nerves; for instance, the superior nerves with the inferior of the brachial flexus, and even the cervical nerves with the pneumogastric. In all these cases there was complete reunion, and in some, a complete return of function. (*See* "Memoirs of the Academy," vol. xiii. p. 14, and his Experimental Researches into the Functions of the Nervous System," &c., p. 272, *et seq.*—*London Lancet,*

Exemption of the Cherokee Indians and Africans from Insanity.—Dr. Lillybridge, of Virginia, who was employed by the Government as the medical officer to superintend the removal of the Cherokee Indians, in 1827-28 and 9, and saw more than twenty thousand Indians, and inquired much about their diseases, informs us he never saw or heard of a case of insanity among them.

Dr. Butler, who has been a devoted missionary and physician among the Cherokees for about a quarter of a century, informs us, in a recent letter, that he has yet seen no case of decided insanity among them, though he has occasionally seen them delirious when sick of other diseases; and adds, that an intelligent chief, a man now 80 years old, told him

that "he had never known a case of insanity among his people, such as he had seen in the Hospital in Philadelphia."

Insanity is rare, we believe, among the Africans. Cinquez, and other of the *Amistad* Negroes, when in this country a few years since, visited the Retreat for the Insane at Hartford, Ct,, and saw many of the patients there. They informed the writer of this article, that insanity was very rare in their native country. Most of them had never seen an instance. Cinquez stated however that he had seen one case.—*American Journal of Insanity,*

On the Incubation of Insanity.—Dr. Clutterbuck, at a late meeting of the Medical Society of London, regarded insanity as not a disease *per se*, but merely a symptom of disordered function of the brain. If we admitted—and, he thought, it could not be reasonably doubted—that the brain was the organ through which the mind was manifested, it followed that every disordered condition of the mind was dependent on some disordered condition of the brain, not always, it was true, obvious or appreciable, but still was clear that the brain was not in a sound state of health. Not always to the extent of disorganization, for it was known that insanity often left the patient for a time, and then recurred, from causes not very obvious. The brain was often found diseased in cases of insanity, but we wanted proof that those changes were always the cause of insanity. Authors of experience, however, had asserted that they had always found the brain diseased in cases of insanity; Sutherland and Haslam were of this number; and Mr. Lawrence, out of seventy-two cases, had found the brain diseased in all, a structural change existing in each case. These facts did not prove that the structural disease was the cause of the symptoms, but it showed that in insanity the brain was not sound. That these conditions were not the proximate cause of the insane phenomena, however, was proved, for they existed independent of insanity. We found opacity of the membranes, increased vascularity, bloody points, induration, softening, and serous effusions of the brain, in cases in which insanity did not exist. Changes, however, might exist beyond what we were at present enabled to discover. What, then, caused this state of the brain? He believed that it was always the result of inflammation which had existed at some

period or other. He thought this because inflammation was the great disorganizing process; and if disorganized, therefore, the brain must, at one time, have been inflamed. The disorganization was the result, in some way, of inflammation. We might often trace insanity in its early stages to the influence of extreme mental emotion, the effects of alcohol, or of local injuries, the insanity subsiding on the subsidence of these causes, so that we had cause and effect at once before us. He complained that the term incubation was not expressive of the manner in which insanity progressed in its early stages. Confirmed insanity was incurable, as the brain had become permanently affected. The time for treatment was in the early stage; subdue the inflammation then, and you subdue the symptom, and the brain regains its natural condition.—*Lancet.*

Jewish Physicians in Hamburgh.—In consideration of the active and generous conduct of the Jews of Hamburgh, and especially of the great banker, Solomon Heine, on occasion of the fire in that city in 1842, their condition has been ameliorated. Before, the laws weighed heavily upon them, as every where else in Germany. For a long while, the Jews of Hamburgh have been rigidly restricted to commerce and to the exercise of the *medical profession*. Now, the Council of Ancients propose to open to them all trades and professions.

What a low estimate, it would appear, has been made of medicine, by the enlightened people of Hamburgh! If the oppressed Israelites studied any science, it must be medicine—as much as to say, it is only a mean occupation, and very well accords with their character and political circumstances. Associated with degradation as medicine is, when practised by a Jew, some of the brightest luminaries of the last century were of that despised, persecuted race. Those who have read Dr. Dunbar's translations of the early efforts and claims of the Jewish physicians of Europe, know very well our indebtedness to them, notwithstanding the low estimate in which they are held by the authorities of Hamburgh.

Boston Med. and Surg. Jour.

Enlargement of the Breasts in a Male: atrophy of the Genital Organs.—A man, aged about 55, originally a quarryman, at Rochester, but afterwards one of the British Legion in Spain, was presented to the Society, having a remarkable developmnt of the breasts. He stated that he had been very healthy, had three children, the eldest 25, the youngest would have been between 8 and 9 years of age;that in an engagement in Spain, he tried to jump over a trench, but, being laden with his knapsack, failed, and fell with his chest against the opposite margin. He received a blow on the lower part of the spine, and falling some depth, received a violent blow on the occiput, which rendered him insensible. He was under the care of Mr. R. Alcock for some time. Since that period the mammæ have gradually increased in size, his voice has diminished in power, his beard has diminished, the right testicle has diminished to a very small size, and the left is still diminishing, and he has had no sexual desire since. He is partially paralysed on the right side, and moves about very much bent. Skin fair and soft; beard very thin and soft; voice weak; mammæ pendulous; nipples rather prominent; the glands very large and distinct; penis small and shrivelled; right testic having the feeling of condensed membrane; left of a moderate size, but not firm, and tender on pressure; the pelvic region round, more like that of a woman, not presenting the angular appearance of the male. He denies having ever had venereal disease, or taken any quantity of mercury.

Cases were mentioned of enlargement of the breasts in the male related in the "Philosophical Transactions," vol. 41; and in Captain Franklin's "Journey to the Polar Regions;" and of emasculation consequent upon blows or wounds of the occiput, related by Larrey, in his "Clinique Chirurgicale; and by Hennen in his "Military Surgery." There was no appearance of abscess iu the mammæ.—*Prov. Med. and Surg. Jour.*

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LONGEVITY OF MEDICAL MEN.

The following note was sent to us by Dr. Boling to accompany his essay on the "Health of the Clergy," but reached us too late for our last number. Its humor will amuse, and its facts are worth preserving. We insert it in this place, and are sure that our readers will thank us for the opportunity of enjoying a laugh. Y.

"As an appendix to the catalogue of female writers of eminence who lived to an advanced age, I give below a short list of distinguished members of the medical profession, arranged in the order in which the names occurred to my mind. They are the only ones for whose ages I have examined. They were all highly distinguished, and although the opinions and doctrines advocated by some of them, may have been incorrect, and may have tended but little to the ultimate advancement of the science, still the genius of the authors gave to their opinions for a time a wide spread popularity. It may be maintained that it was not so much to superior abilities, as to the lengthened term of their lives that we are indebted for their labors. But to this my answer is that, in many instances their

principal works, the foundation upon which the reputations of the authors were built, were written before the attainment of old age—in some instances, in the earlier years of manhood.

	Years.		Years.
Hippocrates, from	85 to 109	Haller,	69
Galen,	70	Harvey,	80
Boerhaave,	78	Sir C. Bell,	67
Cullen,	78	Sir A. Cooper,	73
Sydenham,	65	Abernethy,	66
Brown, (intemperate)	53	Malpighi,	67
Rush,	68	Ruysch,	93
Physick,	70	Morgagni,	89
Broussais,	66	Larrey,	75
Hunter, (John)	65	Darwin,	71

"Shall I add to this list the names of Cervantes and Hahnemann? In regard to the former there is perhaps no good reason for classing him with medical authors, unless we are agreed to take the authority of the great Sydenham as conclusive on this point, who, as is known, when consulted by Sir Richard Blackmore as to what books he should study to prepare him for the medical profession, advised him to "read Don Quixote." The formula for the "precious balsam," prepared by the Knight of the rueful countenance, which produced such wonderful effects on his Squire, gives him further claims to a rank in our profession. This is the only thing in his immortal work that I can at this moment call to mind which can be considered as in any way pertaining to practical medicine. Perhaps it would be more correct to class both him and Hahnemann with the satirists. As Don Quixote was written in ridicule of the extraordinary and absurd ideas prevalent in regard to Chivalry, so in all probability was it the original intention of the sly old German, that his "Organon" should be received as a burlesque upon the different medical doctrines and theories which from time to time had replaced one another. "Many wild and ridiculous theories of medicine," he probably said to himself, "have prevailed, and have been the cause of much sarcasm upon the profession. My satire upon these false systems shall be the severest of all. I will do for medicine what Cervantes has done for chivalry"—and lo, the "Organon" appeared! Had it been received, as must have been the original ob-

ject of the author, the pungent wit expended upon the profession by Le Sage, Smollet, Moliere, and others, sparkling though it was, would have been completely dimmed. The jest of the author was taken as earnest, however, and though his wit was wasted, his pockets were filled. This was probably some consolation for his disappointment, and may have rendered the laugh in his sleeve more merry, till in the years of second childhood, he himself began to believe—received, in other words, that degree of enlightenment which had long since been attained by his proselytes. If the first object for which the work was intended was defeated, it probably advanced as well the authors interest, and served to develope a degree of gullibility in mankind previously unsuspected."

ASAFŒTIDA A PREVENTIVE OF MEASLES.

So Dr. R. B. Harper, of Bloomington, Tennessee, thinks he has discovered, and the facts upon which rests his belief we give below. The first question that will arise is, do the cases cited make out the point—is it true? That is a very essential question which must first be settled in all such cases. The inquiry would naturally afterwards come up, what is the *modus operandi* of the article? Is it homœopathic? Does asafœtida cause measles in the healthy subject? If not, one of two things must be true;—either that Hahnemann missed it, in this instance, or else asafœtida is not a preventive of measles. We shall not discuss any of these subordinate questions until the primary one is put to rest, and in order that other observers may turn their eyes towards the matter, we publish Dr. Harper's cases as they were lately communicated to us. Y.

"In the fall of 1842, Rubeola prevailed in this region of country in a form unusually aggravated. Reasons existed with me why I should be peculiarly anxious to keep the disease out of my family, none of whom had ever had measles, and as asafœtida had been mentioned as possessed of prophylactic virtues, I resolved on giving that article a trial. Accordingly I made a saturated tincture of it, of which I took a table-spoonful three times a day, at the same time carrying a lump of asafœtida in my pocket, and swallowing small pieces of it occasionally. I was particular to take it in some

of these forms just before visiting a case of measles. My family also made free use of the remedy. Among others, I attended the following cases during which time I was exposed to the disease:

"1. I was called, October 11th, to see Mrs. D. whom I found covered with the eruption of measles, and suffering from high fever, nausea and vomiting. In a minute examination which I made of all the features of the case I was fully exposed to the infection, and having up to this time been quite susceptible to contagious affections, I supposed that I had the best chances to take measles from my patient. But nothing of the sort occurred.

"2. October 16th, I was called to visit the child of Mr. J. M. with measles; I remained in the house during the night following, and examined a case of measles in a negro, in addition to the case of the child.

"3. Oct. 20th, I visited Mrs. W. with measles, and found her also in labor. The eruption was fully established, and was attended with high febrile excitement, but notwithstanding these unpleasant circumstances, the labor progressed favorably, and in a short time she was delivered of a well grown child, and healthy in every respect, except that it was covered with measles. I was necessarily much about the patient during her labor, but nevertheless escaped her disease, and thus passed through the epidemic without contracting it. The mother and child in this case both did well.

"I might speculate on these facts, but will leave that task to others. If my experience of the powers of asafœtida in this respect should be confirmed, the discovery will be valuable. The point, however, requires additional investigation. In medicine, everything depends upon experience. Accident has placed in our hands many of our most precious remedies, which, probably, when first proposed, commanded but slight attention, and awakened, if not violent opposition, or merciless ridicule, at best only feeble expectations. But bark, mercury, iodine, and the vaccine virus are agents the potency of which in their appropriate maladies, no one now questions, although no one professes to be able to explain their mode of curing or preventing disease."

A QUESTION OF LEGITIMACY.

Our friend, Professor Cobb, has lately been consulted respecting a case involving a question of paternity, which came up not long since in one of the courts of a Southern State. The facts stated are as follows: A white woman, the wife of a planter of wealth and respectable connexions, gave birth to a male child of so dark a complexion that unpleasant suspicions were awakened among her acquaintances. Her husband died subsequent to the birth of the child, and after remaining a widow four or five years she again married. A doctor, it seems, in an evil hour, charged her with incontinence, alleging that "she had given birth to a mulatto child." Upon this an action for slander has been brought, and is still pending, the jury having failed, on the first trial, to agree on a verdict. Nine doctors appeared as witnesses, who expressed opinions widely variant touching the merits of the case.

It was proved that the first husband, the alleged father of the boy, was a man of fair complexion, being of German extraction, but that his mother and two of his uncles were dark like the child, and that his family in Germany were descended from the Gipsies. It was further stated in the trial, that during the pregnancy of the mother with this child she was repeatedly frightened by reports of "negro insurrections."

The appearance of the boy is described as remarkable. His surface presents different shades of color, the chest and axilla being nearly white, while the abdomen is dark, the change occurring abruptly and being marked by a well defined line. The glans penis "is quite blue," while the other parts of the genital organs are of the complexion of the general surface. The boy has been growing gradually whiter since birth; "his hair is nearly straight—a little curled, but not kinked;" his feet and ankles present nothing of the negro peculiarity; his whole appearance might suggest the thought that he was the "product of a white woman and a mulatto man." Such is the description of the boy's physiognomy.

It was proved by the defendant that the character of the plaintiff had not been above suspicion, and that criminal connexion with her carriage-driver had been possible, if not probable.

Some of the questions upon which the medical witnesses differ are thus put by Professor Cobb's correspondent:

"Is not a mulatto from a white woman darker than one from a black woman?

Are there any anatomical or physiological signs by which negro blood might with certainty be detected?

Is it not usual for mulattoes to grow darker instead of whiter?

Would you place any reliance upon the color of the glans penis?

Do you think it possible that a *nævus materni* could cover nearly the whole body?

Do you think it possible for color to show itself in the third and fourth generation?

Do you know of any mode of bleaching by which the skin might be rendered white?"

The narrator is one of the nine medical witnesses in this suit, and is evidently very much perplexed. He says, "it has been my misfortune since I commenced the practice of physic to be called upon to give evidence in many cases of medical jurisprudence, but never before in one so difficult as this."

The gallantry of our distinguished colleague, no doubt, will tempt him to lean to the side of the lady and the legitimacy of her offspring, and the following facts in the case will support him in that opinion: namely, that the boy has been growing whiter since his infancy; that he is wanting in the characteristics of the negro race about the heel and ankle; that he is descended of the Gipsies in whom the dark complexion is a hereditary quality; and that some of his ancestors were as black as himself. Still it must be admitted that these proofs are not conclusive of the purity of blood, and the question is one which it may be impossible to place beyond the reach of controversy. Nature in her operations in this obscure walk seems not to be governed by very settled laws. Thus we are told of an English woman married to a black man, of whom the offspring was quite black; and of a similar case in which the child resembled the mother in fairness of features, the whole skin being white, "except some spots on the thigh which were as black as the father." White, in his work on the Gradation of Man, mentions a more remarkable case—that of a negress who had twins by an Englishman, one being perfectly black, with short, woolly, curled hair, the other white with hair resembling that of a European. Beck, in his Medical

Jurisprudence, has more cases going to show the difficulty of establishing any universal rule on the subject. Parental likeness in the estimation of Lord Mansfield is one of the strongest arguments in favor of legitimacy. In a case which came before him involving this question he said, "I have always considered likeness as an argument of a child being the son of a parent, and the rather as the distinction between individuals in the human species is more discernible than between other animals. A man may survey ten thousand people before he sees two faces exactly alike, and in an army of ten thousand men every man may be known from another. If there should be a likeness of feature, there may be a difference in the voice, gesture or other characters; whereas a family likeness runs generally through all of these; for in everything there is a resemblance as of feature, voice, attitude and action." This test, we should suppose, might avail in the case under consideration.

But Lawrence in his Lectures cites many facts going to show that the above law of transmitted likeness is not without exceptions. He remarks: "Children do not always resemble their parents; and hence we have occasionally persons produced in each race with characters approaching those of the other races. Among the white races of Europe scattered instances of individuals with skins nearly as dark as those of the Mongols or South-Sea Islanders are not unfrequent. I lately saw a girl," he continues, "whose dark-olive skin and jet-black hair, very much like those of a Chinese, joined to English features, made me suppose that there was some mixture of blood: it turned out, however, that her parents were both English: the mother dark, but not of so deep a tint as the daughter, and the father fair."

Our memory, happily, furnishes us with but one solitary case in which children were born of a white woman by a black man, and in this the offspring were rather bright mulattoes, partaking in the usual degree of the characters of the two races. Such instances, we are convinced, are of rare occurrence; and we hope that the lady involved in the vexatious suit to which we referred at the beginning of this article will be able to satisfy the jury, and society, that her unlucky boy inherits his dark skin from ancestors at least as respectable as the Gipsies.

Y.

BOOKS RECEIVED.

In future numbers, it is our expectation to notice at length several of the following works, but at present we are restricted to the briefest remarks on their merits.

A Treatise on the Diseases and Special Hygiene of Females, by Colombat De L'Isere, translated from the French by Charles D. Meigs, M.D. &c. We have received a copy of this elaborate work from the publishers, Messers. Lea & Blanchard. We hope in our next number to be able to present our readers with a review of it from the pen of one of our most valued correspondents. We have called the work an elaborate one, and the fact that references in it are made to "more than a thousand authorities" justifies that character.

A Practical Treatise on the Diseases peculiar to women, illustrated by cases derived from Hospital and Private Practice. By Samuel Ashwell, M. D. &c, with notes by Paul B. Goddard, M. D. &c. To the same spirited publishers we are indebted for this work, treating of subjects almost identical with those of the volume just above. The publication of two such volumes at the same time, from authors of different countries, is proof of the interest awakened in the profession by the complaints of females. We indulge the hope that the diffusion of so much light as these works from the pens of distinguished practitioners ought to impart, will be beneficially felt by our brethren, and by this interesting class of their patients.

The Principles and Practice of Dental Surgery, by Chapin Harris, M. D. &c. This is a second edition of Dr. Harris' compendious work on Dental Surgery. We are glad to see that the practitioners of that branch of the art appreciate a work which reflects so much credit upon their profession. We received our copy from Messrs. Lindsay & Blakiston, the publishers at Philadelphia. We shall have more to say about it at an early day; but mean time we advise every dentist to avail himself of the earliest opportunity of supplying himself with a copy of it.

Principles and Illustrations of Pathological Anatomy, by J. Hope, M. D. &c., edited by L. M. Lawson, M. D. &c. We are indebted for a copy of this admirable work to the politeness of the editor. The publishers have given it to the readers on a very large type and fair paper, making a volume of imposing size and show. Y.